Expert Report of

David Neumark, Ph.D.

in the matter of

Cahill et al. v. Nike, Inc.

(corrected on August 5, 2021)

Filed with Redactions

I. Introduction

1. I am David Neumark, Distinguished Professor of Economics at the University of California—Irvine. I am a labor economist who has done extensive research on labor market discrimination, including methods for measuring and testing for discrimination that have been adopted by many other researchers. I have published approximately 30 peer-reviewed journal papers on discrimination based on race, ethnicity, sex, or age, in journals including *American Economic Review*, *Contemporary Economic Policy*, *Economic Journal*, *Industrial Relations*, *Industrial and Labor Relations Review*, *International Economic Review*, *Journal of Human Resources*, *Journal of Labor Economics*, *Journal of Policy Analysis and Management*, *Journal of Law and Economics, Journal of Political Economy*, *Review of Economics and Statistics*, and *Quarterly Journal of Economics*, as well as other studies in edited books, and a full-length book on sex discrimination and sex differences in labor markets (based on my papers). The goal of much of this research is to better understand the role of discrimination versus other explanations of differences in labor market outcomes by sex, age, race, or ethnicity.

2. As a labor economist, most of my work involves statistical and econometric analysis of data. As examples, several of my research papers on discrimination focus on the development of new statistical techniques to measure and test for labor market discrimination. My graduate courses in labor economics and my training of Ph.D. students in labor economics focus heavily on econometric methods.

3. I have previously held positions at the Federal Reserve Board, the University of Pennsylvania, Michigan State University, and the Public Policy Institute of California. I am a research associate of the National Bureau of Economic Research, and a research fellow at

IZA (the Institute for the Study of Labor) and at CESifo in Germany. I also co-direct the Center for Population, Inequality, and Policy at UC—Irvine. In 2019, in recognition for my contributions to labor economics, I was elected a Fellow of the American Association for the Advancement of Science.

4. I have been retained by the Plaintiffs as a statistical expert to evaluate claims of gender discrimination at Nike, Inc. (henceforth "Nike"). I am compensated at the rate of $575 per hour.[1]

5. This analysis is based on my current understanding of the data with which I have been provided by Nike. These data files are listed and described in Appendix A of my report. It is possible that I will learn more about the Nike data, company procedures, and other matters in the course of this case, which could lead to changes in the analysis and findings.

6. Other materials I considered are also listed in Appendix A. Appendix E of my report provides an abridged CV listing my publications from the last 10 years. Appendix F of my report details my expert witness work in the last 4 years.

II. Questions I Was Asked to Consider

7. I was asked by the Plaintiffs to consider the following questions:

   a. Did female employees in the Covered Positions receive less compensation than male employees?[2]

[1] In drafting this report, I was assisted by Carl McClain and Valentyna Katsalap (of Employstats) and Elizabeth Maloney (one of my current graduate students at UC Irvine). Their hourly rates are $225, $305, and $105, respectively.

[2] "Covered Positions" are salaried corporate positions at Nike Headquarters in Oregon that are or were lower level positions than Vice-President excluding Nike retail store employees, lawyers within Nike's Legal Department, and employees in Nike's Finance and Human Resources Department. See *First Amended Class and Collective Action Allegation Complaint, filed 11/19/18*, para.'s 176-177, and Paul Hastings December 30, 2019 Letter re: *Cahill et al. v. Nike, Inc.* – Production of Nike Data Files.

b. Did female employees who were employed in the Covered Positions receive lower starting salaries than male employees?

c. Did the consideration of prior pay of a new hire or the increase in pay that a new hire would receive when setting starting pay disadvantage female employees?

d. Did Nike's practices for setting merit increases disadvantage female employees?

e. Did Nike's practices for awarding bonuses pursuant to its Performance Sharing Plan (PSP) disadvantage female employees?

f. Did Nike's practices for assigning new hires to Job Levels disadvantage female employees?

g. Did female employees in the Covered Positions receive fewer promotions than male employees?

III. Summary of Findings

8. The data tell a straightforward story, with the following key conclusions:

a. Women and men come to Nike with similar human capital (education, experience), and women get performance ratings as high or higher than men.

b. Women are hired at lower Job Levels than are men who come to Nike with similar human capital.

c. Once hired, women are promoted more slowly than men; this is driven by non-competitive promotions.

d. The lower Job Levels at hire, and the slower progress to higher Job Levels, creates a large pay shortfall for women in the class period.

i. A simple estimate of this pay shortfall is $11,363 lower pay (in December 2019 dollars) per woman per year.[3]

e. In addition, women doing substantially similar work to men, and with similar qualifications to men and similar performance to men, are paid less at hire. This starting pay gap persists into the class period, during which women are paid less than men who are doing substantially similar work and have similar qualifications and performance. This happens because, after being hired, subsequent pay increases for women and men with similar performance are based on the same percentage pay increase, perpetuating the starting pay gap.

i. The pay gap within the same job creates a large pay shortfall for women in the class period. A simple estimate of this pay shortfall is $3,138 lower pay (in December 2019 dollars) per woman per year. Note that these amounts, while sizable, are smaller than the amounts in paragraph 8.d.i. above, because they do not reflect the female pay shortfall attributable to women being hired at lower Job Levels and being promoted more slowly.

9. The Summary Table below provides a summary of the key analyses included in my report that support these conclusions.

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[3] These shortfall estimates are not estimates of damages. Such estimates will be refined at a later date. Notably, the shortfalls I have calculated in this report do not include data analysis beyond September 2019, calculated interest on wage loss, all potential lost compensation caused by initial job assignment and promotion discrimination not included in the shortfall analysis, including equity awards, or estimates for liquidated and/or punitive damages, which I have been informed should be included in a comprehensive damages estimate.

Summary Table: Key Findings

A. Female Class Period Pay Difference	Pay Difference	Standard Deviations	Implied $ Difference (per woman per year)
Controlling for Job Subfamily (Table 2, col. 2)			
Base pay	-6.4%	12.1	-$8,484
Bonuses	-12.5%	11.2	-$2,516
Combined	-7.4%	12.3	-$11,363
Controlling for Job Subfamily-Job Level Interactions (Table 2, col. 4)			
Base pay	-1.8%	9.0	-$2,371
Bonuses	-3.2%	5.7	-$645
Combined	-2.0%	9.6	-$3,138

B. Female Starting Pay Difference	Pay Difference	Standard Deviations	Implied $ Difference (per woman per year)
Controlling for Job Subfamily (Table 7, col. 3)			
Base pay	-3.3%	3.8	-$3,852
Controlling for Job Subfamily-Job Level (Table 7, col. 4)			
Base pay	-1.2%	3.3	-$1,368

C. Female Promotion Rate Difference	Promotion Rate Difference (percentage)	Standard Deviations
Non-competitive promotions (Table 10, col. 3)	-6.54%	2.5
Non-competitive promotions, 2 or more levels (Table 11, col. 1)	-11.2%	3.2
Non-competitive promotions, 3 or more levels (Table 11, col. 2)	-12.4%	2.6

D. Starting Job Level (1-11)	Job Level Difference	Standard Deviations
Job Level Differences	-0.19	2.9

IV. Data

10. I was provided with a number of data files. Here, I describe the files that I use in my analysis.

a. The main data file on employees over time is *Snapshots_201207_201909_Attorneys_Eyes_Only.txt*. This file contains data on each employee in each month, from July 2012 through September 2019. It includes information on the employees' Job Levels, Job Functions, Bands, hire dates, employee name, salary, and more. It has an employee ID variable (which I refer to as "*Personnel ID*") that allows the data to be linked to other files. I refer to this as the "Snapshot data," because it provides a snapshot of employees on the first of each month. This file covers all individuals who worked in covered positions in the class period.[4]

b. An additional file on employees is *Static_Table_20190831_Attorneys_Eyes_Only.txt*. This file contains one record per each employee, and their status (e.g., still employed or not) as of August 31, 2019. This data file includes information on gender, birth date, termination dates, and hiring dates, and has the same *Personnel ID* linking variable. This file covers all individuals who worked in covered positions in the class period.[5] I refer to this as the "Static file."

c. Data on bonuses and merit increases to base pay awarded during the class period come from the third through fifth files: *Comp_Change_thru_20190531_Attorneys_Eyes_Only.txt*, *Comp_Change_2019_PSP_Attorneys_Eyes_Only.txt*, and

[4] Paul Hastings December 30, 2019 Letter re: *Cahill et al. v. Nike, Inc.* – Production of Nike Data Files.

[5] Paul Hastings December 30, 2019 Letter re: *Cahill et al. v. Nike, Inc.* – Production of Nike Data Files.

Comp_Change_2019_Merit_Attorneys_Eyes_Only.txt ("Comp change," "PSP," and "Merit file(s)"). The first file includes Equity, Merit-Based Increases, and PSP Bonuses; the latter two files provide data on PSP bonuses or merit increases to base pay only for the remainder of 2019 not covered in the first file. These files cover all individuals who worked in covered positions in the class period.[6]

d. I obtain data on additional performance related measures (Potential Appraisal Ratings and Talent Segmentation Ratings) from the files: *Pot_Appraisal_FY13_FY18_Attorneys_Eyes_Only.txt* and *Segmentation_FY18_FY20_Attorneys_Eyes_Only.txt*.

e. A separate file, *Taleo_Hires_Data_Attorneys_Eyes_Only.txt* ("Hire data"), provides a unique record for each external and internal candidate Nike hired for a posted job requisition from 2012 through 2020 (what Nike internally calls "competitive" promotions).[7] The file contains information on the job hired into, the hire date, gender, and whether the person was an internal or external hire. For those who are internal hires, *Personnel ID* is available to link to the other files (about two-thirds of the observations). These hire data are limited to those who were in a covered position for which there was a requisition for a job opening.[8]

f. Finally, *Taleo_Application_Data_2012_Attorneys_Eyes_Only.txt* through

[6] Nike also produced *Comp_Change_2019_Equity_Attorneys_Eyes_Only.txt* , which contains data about equity awarded to individuals who worked in covered positions for part of 2019. Paul Hastings December 30, 2019 Letter re: *Cahill et al. v. Nike, Inc.* – Production of Nike Data Files.

[7] Nike has used Taleo for competitive job changes based on posted job requisitions. It is not used for non-competitive promotions. Non-competitive promotions are those promotions in the Snapshot data file that do not appear in the Taleo Hires data file. (Thomas Dep. 41:24-43:7, and Paul Hastings November 12, 2020 Letter re: *Cahill et al. v. Nike, Inc.*, Case No. 3:18-cv-01477-JR – Your questions regarding Nike's Taleo data production.)

[8] Paul Hastings November 1, 2020 Letter re: *Cahill, et al. v. Nike, Inc.* – Production of Taleo Data Files.

Taleo_Application_Data_2020_Attorneys_Eyes_Only.txt ("Application data") contain records for each application made to a posted job requisition for positions at Nike between 2012 and 2020 (e.g., hires and competitive promotions). This includes information on the job to which they applied, as well as gender, the date of the application, name, work experience, and education. These application data are for covered positions for which there was a requisition for a job opening.[9]

11. My analysis of pay differences for this case relies on the Snapshot data coupled with the Comp change and PSP files.

12. I also study starting pay, which has data available for those who began working in 2012 or later. My analysis of starting pay also utilizes, when possible, data on education and prior work experience when people began working at Nike. Matching records to the latter information is complicated because that information comes from the Application data, in which the *Personnel ID* identifier that could be used to match directly to the Snapshot and Comp change files is not always populated.[10] However, there is an identifier in the Hire data that matches to an identifier in the Application data (*Candidate ID*), and, as just discussed, the Hire data and Application data sometimes have *Personnel ID*. I match on the latter when I can, but otherwise match on name.[11]

13. I use the Snapshot data to study promotions, including the Taleo hire data to identify competitive promotions.

[9] Paul Hastings November 1, 2020 Letter re: *Cahill, et al. v. Nike, Inc.* - Production of Taleo Data Files.

[10] This would never be populated for someone who was not a Nike employee and applied for a job once. For a non-employee who applied but was not hired, but then was hired later as a result of another application, the *Personnel ID* sometimes gets populated even for the earlier applications. It also gets populated most of the time for Nike employees who apply for other jobs. Paul Hastings November 12, 2020 Letter re: *Cahill et al. v. Nike, Inc.*, Case No. 3:18-cv-01477-JR – Your questions regarding Nike's Taleo data production..

[11] I do this using the *matchit* algorithm in Stata.

14. The different data sets, including the number of observations, are summarized in Table 1.

V. The class and collective action

15. The class in this case is defined as: "All female current and former Nike employees at Nike Headquarters in Oregon, who were employed by Nike at any time from October 11, 2017 through the resolution of this action for claims under Title VII, and for the period from August 9, 2017 through the resolution of this action for claims under ORS 652.220 and ORS 659A.030, in a salaried, corporate position that was or is a lower-level position than Vice-President ("Class Definition," "Class," or "Class Members")." The Equal Pay Act collective action in this case is defined as: "All female current and former Nike employees at Nike Headquarters in Oregon, who were employed by Nike at any time from three years prior to opting-in through the resolution of this action, in a salaried, corporate position that was or is a lower-level position than Vice-President." Excluded from the class and collective action are: "Nike retail store employees, lawyers within Nike's Legal department and employees in Nike's Finance and HR departments."[12]

16. Based on the data I was provided in the Static File, there are approximately 4,607 class members who were employed at Nike Headquarters in Oregon in a salaried, corporate position that was a lower-level position than Vice-President between October 11, 2017 and August 31, 2019. (There are 6,079 males who otherwise meet the same criteria.) The hypothetical maximum of women who could have met the Equal Pay Act collective action definition, between August 9, 2015 and August 31, 2019, totals approximately 5,428. (There are 7,075 males who otherwise meet the same criteria.)

[12] *First Amended Class and Collective Action Allegation Complaint, filed 11/19/18*, para.'s 165-166, 176-177.

17. With regard to liability period, I have been informed by counsel that: (a) The liability period under the Equal Pay Act potentially commences on August 9, 2015, for certain female employees who have opted in to the EPA collective action; (b) the liability period under Title VII commences on August 8, 2016; and (c) the liability period for the Oregon laws commences on August 9, 2017.

VI. Pay Differences in the Class and Collective Action Periods

18. My first analysis examines whether there is a gender gap in pay during the class and collective action periods ("class period"), which extend from August 9, 2015, to the present (although the Snapshot data currently extend only through September 1, 2019). The goal of my analyses is to estimate whether there is a gender difference in pay once I control for individual differences across workers, and in differences in the jobs in which they work. I also report similar analyses for the different liability periods referenced above, for which the conclusions are the same.

19. To estimate whether there are gender disparities consistent with gender discrimination in pay, I estimate regression models for pay. The data used in these models are records for individuals in specific years. These records include different compensation measures, and an indicator for the gender of an employee. They also include measures of the type of job a person at Nike does, and some information on the worker, including the worker's performance review scores and their job tenure at Nike (how long they have been at Nike); other available information on workers is described below.

20. The regression models estimate the gender gap in pay once I adjust for possible differences between female and male employees that could potentially account for this pay gap. For example, suppose that I simply compare average pay of all female and male employees at

Nike, and find that average pay of female employees is 10% lower. It is possible that women do different jobs than men, and those jobs could pay less. It is also possible that women and men are in broadly similar jobs, but the women have lower values of measures that could be related to productivity, such as less tenure or lower performance ratings. In either case, our intuition would be that the 10% estimate overstates the pay gap for comparable women and men in comparable jobs, and we should hence adjust for these differences between women and men before estimating the gender gap in pay that is unexplained by these differences and hence is consistent with discrimination. Of course the opposite is equally possible a priori; women could, for example, have higher performance ratings, in which case the 10% estimate understates the pay gap for women and men who have similar performance ratings and work in comparable jobs.

21. This is precisely what a regression model does. A regression model "holds constant" or "controls for" these other factors. These phrases mean that, in estimating a regression model, I adjust the pay gap for differences in the jobs employees hold, and differences in measures related to their productivity, such as tenure and performance ratings, so that I am comparing pay between comparable women and men in similar jobs. In the example above, it is possible that the 10% gender disparity is fully explained by these other factors, in which case the estimated gender pay gap from the regression would be zero.[13] Thus, my analysis asks – in a detailed manner making extensive use of data provided by Nike – whether other factors such as job tenure or performance can explain any pay gaps by gender that I find.

[13] It is important to point out, though, that it is also possible that the estimated gender pay gap would be larger than 10%, if women are on average in higher-paying jobs or have higher skills. We cannot know, before looking at the data and estimating the regression model, whether other factors controlled for in the regression will lead to a lower or a higher estimated gender gap in pay.

22. It is important to point out, though, that the largest data set provided to me to study pay in the class period includes some but not all of the standard controls for worker characteristics that can help explain pay. Specifically, it does not include a measure of education, nor does it include a direct measure of work experience (which would be constructed, at a point in time, as the sum of prior work experience plus time at Nike). Because I cannot construct an experience measure, I control for age instead.[14] I do have data on time at Nike ("tenure"), and control for it. I also construct a control for time in the job (i.e., the specific job).[15] In addition, as one usually does when controlling for experience and tenure, I include squared terms for age and tenure (and I do the same for time in job). These squared terms allow for the possibility that the effect of experience or tenure diminishes at higher values of these variables.[16] And I have data on performance review scores. Moreover, I do have data on education and prior experience for the subsample of observations with starting pay information. I am able to show that using these more-detailed control variables does not have a material impact on the female pay shortfalls I estimate for the class period. This

[14] A common alternative when a detailed experience measure is not available is to construct potential experience, defined as age minus years of schooling minus 6 (which measures how much one could have worked having gone to school continuously and progressing one grade each year, and working continuously afterwards). However, I do not have a schooling measure. Using a potential experience measure assuming the same year of school leaving for everyone yields the exact same estimated gender differential (because either way of estimating the model introduces a linear and a quadratic term in age).

[15] From the point of view of labor economics, this is less standard than a tenure control, but that is in part because the data sets labor economists usually study do not have detailed information on the specific job at a company that a worker holds. Nonetheless, time in job may have an effect similar to time at the company (tenure) – capturing the accumulation of skills specific to that job that increase productivity and hence generate earnings growth (although, as pointed out in the next footnote, the apparent effect of tenure (or time in job) could arise from other reasons).

[16] The diminishing effect of experience (age) on earnings is predicted by the human capital model and confirmed by the evidence. When there is a diminishing effect, the estimated coefficient on the standard (linear) experience term will be positive, but the estimated coefficient on the squared term negative. For the original treatment of experience, see: Mincer, Jacob. 1974. Schooling, Experience, and Earnings. Cambridge: National Bureau of Economic Research, Inc. Predictions for the effects of tenure are less clear theoretically, in part because there are other theories of why earnings increase with tenure aside from the human capital model (and there are really no predictions for time in job). Altonji, Joseph G., and Robert Shakotko. 1987. "Do Wages Rise with Job Seniority?" *Review of Economic Studies*, Vol. 54, No. 3, pp. 437-59. And in fact in my estimates, there often is not this "concave" shape with regard to tenure or time in job.

implies that the lack of these detailed controls for the more complete class period data set does not bias my estimated female pay shortfalls using the larger data set.

23. If there is evidence that women are compensated less than comparable men from the regression estimates, this evidence is consistent with pay discrimination against women. This conceptualization of pay discrimination is standard in the labor economics literature, beginning with the seminal work of Becker (1957),[17] who defined discrimination in pay as unequal pay for equally productive workers. The use of regression models like those I describe above to estimate gender disparities in pay, in order to assess whether there is evidence consistent with pay discrimination, is pervasive in economics, with scores if not hundreds of papers written in recent decades.[18]

24. The regression models I detail in this report provide estimates of the approximate percent difference in pay between women and men. It is common in the labor economics research literature to use regression models for pay that estimate the effects of different variables – most importantly, in this case, gender – on the percentage difference in pay rather than the absolute difference.[19] This convention, and the reasons for it, goes back to the original development of the earnings regression in labor economics (Mincer, 1974).[20] This is usually done by measuring pay in terms of the "natural logarithm," in which case the coefficient estimates approximate percentage differentials.

[17] Becker, Gary S. 1957. The Economics of Discrimination. Chicago: University of Chicago Press.

[18] See, for example: Altonji, Joseph G., and Rebecca M. Blank. 1999. "Race and Gender in the Labor Market." In Ashenfelter and Card, eds., Handbook of Labor Economics, Vol. 3, Part C, pp. 2943-3630. Amsterdam: Elsevier.

[19] For example, if a woman earns $9,000 and a man earns $10,000, the absolute difference in pay is a $1,000 pay disparity, and the percentage difference for women relative to men is 10% ($1,000/$10,000).

[20] Mincer, Jacob. 1974. Schooling, Experience, and Earnings. Cambridge: National Bureau of Economic Research, Inc.

25. While my regression models estimate a gender gap in pay, we also have to ask whether the estimated gender gap is "statistically significant." The statistical significance of an estimate tells us how likely it is that we would have obtained the estimated gender gap in pay if in fact the true effect of gender on pay was equal to zero. If the estimated gender gap in pay is statistically significantly different from zero, we are more sure that we did not get a non-zero estimate by chance, but rather because there is in fact a gender gap in pay. To assess this, statisticians compute the "standard deviations" of an estimate – in this case, the estimate of the gender gap in pay – and summarize the estimated gender gap in pay in terms of "standard deviations." This "standard deviations" metric is used to determine whether the measured difference in pay between women and men is statistically significant and differs strongly from the null hypothesis of gender-neutral pay setting (i.e., no difference in pay between women and men), which is what we would expect in the absence of discrimination. The more standard deviations from the null hypothesis of zero that the estimated pay gap is, the less likely it is that the estimated gender gap in pay is due to chance, as opposed to a systematic difference in pay between women and men.

26. For purposes of comparison, in most contexts a difference of 1.96 standard deviations would be statistically significant at the 5% level, meaning that the likelihood of observing this value if compensation was neutral with respect to gender is 1 in 20. A difference of 2.58 standard deviations would be statistically significant at the 1% level, meaning that the likelihood of observing this value if compensation was neutral with respect to gender is 1 in 100. Similarly, the likelihood of observing a difference of more than 3.30 standard deviations would be less than 1 in 1,000. A disparity of two standard deviations is generally sufficient to show that a result is extremely unlikely to be caused by chance (less than a 5%

probability, or equivalently less than a 1 in 20 chance). Courts have recognized the relevance of a 5% (or approximately two standard deviations) measure of statistical significance.[21]

27. To provide more detail for even higher standard deviations, the following table shows, for different numbers of standard deviations, the probability that the resulting estimate could have occurred under the null hypothesis of no discrimination (i.e., a true gender gap of zero). If the reported standard deviations in my report are higher than the numbers in the first column of this table, then the probability is less than the numbers shown in the second column:[22]

Standard deviations	Probability
1.96	1 in 20
2.58	1 in 100
3.29	1 in 1,000
3.89	1 in 10,000
4.42	1 in 100,000
4.89	1 in 1 million
5.33	1 in 10 million
5.73	1 in 100 million
6.12	1 in 1 billion

[21] *Hazelwood School District v. United States*, 433 U.S . 299 (1977). This case is typically interpreted as establishing a benchmark of two or three standard deviations. In practice, in empirical economic research, two standard deviations (or less than a 5% probability) is by far the most common benchmark used.

[22] For example, for 9 standard deviations, the probability would be less than 1 in 1 billion.

28. My analysis uses three measures of pay – base pay, annual cash bonuses (PSP bonuses), and then the sum of base pay plus PSP bonuses.[23,24] The sum is my most comprehensive measure of pay.[25] I index all earnings data to December 2019 values using the CPI-U (the "All Urban Consumers Consumer Price Index.[26]

29. As shown in Table 2, I begin with a simple regression that includes a dummy variable for women, and year dummy variables (to capture any pay differences across years aside from

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[23] PSP ("Performance Sharing Plan") bonuses were available in two files, *Comp_Change_thru_20190531_ Attorneys_Eyes_Only.txt* and *Comp_Change_2019_PSP_ Attorneys_Eyes_Only.txt.* The first file contains a detailed breakdown of compensation changes, including the different components of each PSP bonus awarded. Based on the data for this period, it is my understanding that each PSP bonus is the sum of what Nike refers to as a "team" component and a "discretionary" component, which included a percentage modifier based on the employee's CFE rating. (Ex. 601, "FY18 PSP Brochure," NIKE_00013313.) The second data set, which contains information on PSP bonuses awarded exclusively in 2019, only lists the Final PSP total without any "discretionary component." This is consistent with Nike having moved to One PSP, in which the performance modifier was set at 100% for all employees. (White Dep. 109:24-110:4.) Under either iteration of the PSP bonus, the amount of the bonus for an employee has been calculated by taking a percentage of base pay. (White Dep. 123:22-124:6, and "FY16 PSP 2-Factor Payout Examples" NIKE_00030282-83, April 8, 2016.)

[24] There are approximately 3,800 fewer observations in my analysis when PSP bonuses are considered. In most of these cases, the PSP data are missing. These missing cases do not appear to be cases where PSP bonuses equal zero, as there is a small number of cases (86) with PSP bonus coded as zero. Because my analysis uses the natural logarithm ("log") of pay, as is standard in labor economics analysis of compensation, and the log of zero is undefined, I drop the handful of cases with PSP equal to zero. I have verified that this has no bearing on the results.

[25] Equity (or options) grants are awarded to employees in the E band or higher. Although Nike documents stipulate that employees in E band and higher are eligible for stock awards ("Talk With *me*: Stock Options," NIKE_00030274, April 21, 2016), at least in 2016, the data show that 55 employees in the U band received stock awards. Nike documents indicate that equity compensation is driven by CFE ratings. For example, for the 2015-2017 period: "The CFE Rating drives decisions on … Stock Option awards, so in this way, the employee's performance directly influences their total compensation." (Ex. 603 at NIKE_00026618.) In addition to CFE rating and Band, it appears that Leadership Potential Appraisal was also a factor – or at least whether the employee was rated as high performance/high potential because the stock award collection templates contained a yes/no flag for whether employee was rated in Leadership Potential Appraisal as "high performance/high potential." (White Dep. 59:15-60:6.) And for 2018 on, the following factors are supposed to be considered when determining which amount to select: "prior year performance, future potential, impact of loss and risk of loss." (Ex. 559 at NIKE_00003325; also NIKE_00033960 at -33969 ("Stock awards are pre-populated based on the employee's country and band. Managers can further invest by selected from a predefined dropdown list and taking into account prior year performance, future potential, impact of loss, and risk of loss.").) Given that I control for performance ratings in the models I estimate, were I to estimate models for equity compensation, there would not be a reason to expect a gender disparity for women and men in the same job. However, my analysis shows that women are hired at lower Job Bands and Job Levels than are men, and are promoted more slowly. Since equity compensation is used at higher Job Bands, it logically follows that women would be disadvantaged with regard to equity compensation by being hired at lower Job Bands and Job Levels and being promoted more slowly.

[26] See https://www.bls.gov/cpi/data.htm.

those related to general price changes).[27] I also include information on the controls I have that are suggested by standard labor economics, and available in the data – age (as a proxy for experience) and its square, tenure at Nike (and its square), time in job (and its square), and performance ratings.[28] I estimate a regression using the natural log of the earnings measures, in which case the estimated coefficient of the dummy variable for women gives an approximate percentage difference in pay. Because these estimates are *always* negative in my analysis, I report this percentage difference as a "Female shortfall" – i.e., the percentage by which women are underpaid in a regression model with the controls I included.

30. In Table 2, in addition to reporting the female shortfall, I report two other numbers.

31. First, I apply the percentage female shortfall to average male earnings to compute the implied shortfall in dollars ("Implied $ shortfall"). Note that this calculation does not try to apply the percentage female shortfall to a particular comparator or set of comparators. Note also that this pay shortfall is *per woman per year*.

   a. Second, I report the standard deviations of the estimate. This is relevant to assessing

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[27] A "dummy variable," also known as an "indicator" variable, is a variable that captures a discrete feature of an observation, such as whether the observation is on a woman vs. a man, whether an observation is from a particular year, whether an observation is for a particular job, etc. Dummy variables are used to capture differences like these in the data, as opposed to differences that can be measured on a scale – like years of education or tenure.

[28] I use the Coaching for Excellence (CFE) ratings in the field *RTG_DESC*. This has six different values (in addition to No Rating): Too New to Rate, Unsatisfactory, Inconsistent, Successful, Highly Successful, and Exceptional. (See, e.g., Ex. 534, "Annual Pay Review Key Terms," NIKE_00003273 at -3278; Ex. 638, "Assessing Performance Summary," NIKE_00002301.) I use dummy variables for each of these ratings; I combine the No Rating, Too New to Rate, and cases with missing values as the reference category. According to Nike documents, CFE "provides a consistent approach to evaluating and rewarding performance." (Ex. 500, "Total Rewards Fundamentals: Managing Pay at Nike," NIKE_00003191, Slide 16.) This was described as an accurate description by Treasure Heinle. (Heinle Dep. 81:20-82:22.)

I have verified that the estimates in Table 2 are virtually unchanged if I also include controls for Talent Segmentation and Potential Appraisal Ratings (with dummy variables indicating when these are missing). My understanding is that these latter ratings are more related to other decisions, like promotions. However, it is not surprising that neither the CFE ratings nor these other ratings do nothing to explain female pay shortfalls (or other disparities adverse to women that I document below). Tables B1 and B2 in Appendix B – discussed in more detail below – show that women generally get better performance ratings than men (and never significantly worse ratings than men), conditional on the other controls I include in the models for the outcomes I study.

the statistical significance of the estimated shortfall. As noted above, a standard deviations figure of two (1.96, to be precise) or above corresponds to an estimate that is significant at the 5% level (and as the standard deviations increase, the estimate is more strongly significant).

32. In column 1 of Table 2, with the year dummy variables and worker controls, the estimated female shortfall in base pay is 10.86%, and 17.8 standard deviations (SDs) – strongly statistically significant.[29] The implied dollar shortfall is $14,464, which is the shortfall per woman, per year (in December 2019 dollars). For bonuses, the estimated female shortfall is 19.41% (15.49 SDs), an annual shortfall of $3,923.[30] And for the sum of base pay plus bonuses, the estimated female shortfall is 12.01% (17.30 SDs), an annual shortfall of $18,568.[31]

33. The implication of the estimates in column 1 is lower pay of women at Nike is not explained by women receiving lower performance ratings, having been at Nike for less time (lower tenure) or in their specific job at Nike for less time, or being younger (or less experienced).

[29] I refer to these estimates as percentage differences. In fact, for log earnings equations, the estimated coefficients only provide an approximation to the percentage difference, but for the magnitudes reported here, the approximation is very close. To be precise, the percentage difference would be calculated as exp(b) – 1, where "exp" is the exponential function, and b is the estimated coefficient of the female dummy variable. For example, for $b = -.05$, the percentage difference would be 4.88%. (See: van Garderen, Kees Jan, and Chandra Shah. 2002. "Exact Interpretation of Dummy Variables in Semilogarithmic Equations." *Econometrics Journal*, Vol 5, No. 1, pp. 149-59. They also point out that this is actually slightly more complicated if we try to account for the fact that b is an estimate of the female pay shortfall, rather than a known quantity.)

[30] Note that in my analysis of bonuses, the sample is smaller by about 3,800 observations. These appear to be cases in which employees were not eligible for bonuses (in a particular year). About 74% of these cases were employees hired or re-hired after May 31 of the corresponding year. In order to be eligible for a PSP bonus an employee must have been employed on May 31, the "last day of the annual performance period," and cannot have a performance rating of "unsatisfactory." (White Dep. 17:14-17; Ex. 500, "Total Rewards Fundamentals: Managing Pay at Nike," NIKE_00003191, Slide 19 notes confirms that "employees must be hired on or before May 31st in order to be eligible for a bonus payout.) Moreover, employees will not receive a PSP bonus if they were "terminated for a performance issue or a violation of company rules prior to the payout" in August." (White Dep. 17:17-21.)

[31] In the remainder of this section, I continue to report the separate bonus results in the tables, but I only discuss the results for base pay and for base pay plus bonuses. Later, the report discusses the evidence on bonuses separately.

a. Underlying these findings are three facts that are reported in Panel D of Table 3. Women on average have higher tenure ([redacted] years), time in job ([redacted] years), and higher performance ratings (e.g., [redacted] with an Exceptional rating, and [redacted] with a Highly Successful rating, or, [redacted] of women in the highest two ratings categories, vs. [redacted] of men), while men are on average older.[32] This is reflected in the impact of including these control variables on gender differences in pay. Because women have higher tenure, more time in job, and higher performance ratings, adding these controls increases the gender gap in pay (columns 1-4 of Table 3). In contrast, adding age reduces the gender gap in pay (column 5 of Table 3).[33] (When all of these controls are added simultaneously, we get back to column 1 of Table 2.)

b. In Table B1, in Appendix B (which includes some supplemental tables and a supplemental figure), I report a more-detailed analysis of gender differences in performance (CFE) ratings, including the same control variables (except, of course, the performance ratings) as in the class period pay regressions. These regressions show that women receive higher performance ratings than similar men in similar jobs. In column 1, I order the rankings from highest (5, for "Exceptional," to 1, for "Unsatisfactory"). I omit those coded as "No Rating," "Too New to Rate," or missing rating, since I do not know how to rank these. In the remaining columns, I report estimates for linear probability models for receiving a given rating or higher.

[32] I report these differences for the sample in Panel A of the table, which is larger because of missing data on bonuses. However, the same relative comparisons for women and men hold for the sample in Panels B and C, as the table shows.

[33] Strictly speaking, these statements hinge on, e.g., women having higher tenure conditional on all the other controls in the model (or the means of these variables "conditional on" the other controls), which is *not* what the lower panel of Table 3 shows. However, by seeing what occurs in the regressions when these controls are added, we know that the differences in the unconditional means holds true of the conditional means.

We find that women receive significant higher ratings. In column 1, this is evident for the full set of ratings. In columns 2-5, I find that women are likely to get higher ratings in every case, with the estimated differentials in columns 3, 4, and 5 statistically significant (> 2 SDs). (Table B2 shows that this is also true – women receive higher ratings – for the Talent Segmentation rating. For the other three ratings I have and examined – Risk of Loss, Impact of Loss, and Leadership Potential Assessment – there are not significant gender differences.)[34,35]

c. Table 3 shows the impact of these different controls, and the average values of the controls for men and women. First, the bottom of the table shows that women have higher average tenure and get higher performance ratings, while men are a little older. Second, the regressions in columns 2-5 add different combinations of controls

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[34] Nike has utilized several potential assessment practices, including Leadership Potential Assessment (LPA), Talent Segmentation, Risk of Loss, and Impact of Loss.

Prior to FY2019, Nike used LPA to assess performance and potential of Nike employees. It was based on a nine-box grid with performance trend (hi, mid, or low) on the vertical axis and leadership potential (hi, mid, or at) on the horizontal axis. (Ex. 650,"Performance Potential Education Deck" at p. 5; Heinle Dep. 236:11-237:19.) LPA applied to employees in E and S bands and to some employees in U band. (Heinle Dep. 163:11-20.)

Talent Segmentation, which replaced LPA sometime in calendar year 2018, classified employees into four segments based upon various performance and leadership criteria. (Ex. 644, "Facilitation Materials" at p. 35, identifying the four segments as "transitioning talent," "expanding talent," "advancing talent," and "accelerating talent.) Talent Segmentation, like LPA, focused upon employees in bands E and S. (Ex. 645, "Talent Segmentation One-Pager," NIKE_00013810.) The fact that LPA and Talent Segmentation Ratings are available for different periods makes it harder to incorporate them into regression models. Still, as noted earlier in discussing the robustness of the results in Table 2 to including these variables, I do this by including dummy variables for when the ratings are missing.

Nike also has used Impact of Loss, defined as "[t]alent with capabilities, knowledge, or experience that stand out differentially when compared to peers," and Risk of Loss, defined as "[t]alent who is likely considering the idea of leaving Nike due to any number of engagement reasons." (Ex. 649, "Talent Segmentation Guide.") In the data produced to me these ratings are indicated with either a "yes" or "no."

There is some evidence to indicate that Nike may have used some of these potential assessment practices for pay decisions at times (i.e., Impact of Loss and Risk of Loss) or promotion decisions at others (LPA and Talent Segmentation). (See Ex. 644, Slide 5, stating that the purpose of Talent Segmentation is to "allow us to develop, engage and retain our highest potentials – those who will serve as future Enterprise leaders AND our key experts in the business who provide us with competitive separation"; and Ex. 500, Slide 40, indicating that Risk of Loss and Impact of Loss are considered for pay reviews in 2019 but no longer for promotions or lateral moves.)

[35] As the table indicates, I code a "high" Leadership Potential Assessment in two different ways – high on both components, or high on at least one component and mid on the other – and the result is similar (see columns 7 and 8).

(tenure, performance ratings, and age) to the specification in column 1, which does not include these controls. As the table shows, when tenure and/or performance ratings are added (columns 2-4), the female pay shortfall grows, reflecting women having higher tenure and performance ratings, and these factors positively affect pay. In contrast, adding age (column 5) results in a slightly lower female pay shortfall, because men are older and age on average positively affects pay.

34. Returning to Table 2, I next add control variables that reflect differences among workers that can be at least in part influenced by decisions Nike makes. In this part of the analysis, it is important to understand the classification and hierarchy of jobs at Nike.

35. One dimension of how NIKE has classified all Covered Positions throughout the class period is by a "system of pipeline, family, subfamilies," which is described as "very much like the Dewey Decimal system at a library. Help to organize our job codes."[36] Pipelines are also referred to as Job Functions,[37] the language I use henceforth.

   a. Job Functions are the broadest categories: Job Functions are "broad categories of work that can be logically grouped together based on having similar characteristics or prerequisite skills."[38] There are 24 Job Functions in the Snapshot data (with 19 remaining when restricted to class members).

   b. Job Families are groupings within Job Function, of which there are 143 in the Snapshot data (with 98 remaining in the sample when restricted to class members).

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[36] See notes in Ex. 507, Slide 10, "Nike Job Architecture," *NIKE_0023548.pptx*. There are "consistent job code titles, bands across businesses, geographies …." (Stuckey Dep. 58:18-24.) Nike's job architecture framework used for job descriptions has been in place since at least 2010. (Stuckey Dep. 42:23-43:4.)

[37] Walker Dep. 45:12-13.

[38] Ex. 500, Slide 25, "Total Rewards Fundamentals: Managing Pay at Nike."

c. Job Subfamilies are groupings within Job Families, of which there are 257 (with 176 remaining in the sample when restricted to class members).

36. Nike also groups jobs into VALUES bands that are described as a "uniquely Nike structure used to organize jobs throughout the company."[39] Band V is the lowest band and S is the highest. Bands V and A include jobs that are paid on an hourly basis,[40] and bands L, U, E and S contain jobs paid on a salaried basis.[41] The class definition (see para. 15) is limited to salaried employees, who worked in jobs within bands L through S at some point during the class period.

a. The "band structure at Nike creates a common framework for jobs across the company."[42] Bands V and A are referred to as "support bands," bands L through E are referred to as "professional bands," and band S is referred to as an "executive successor pool."[43] This common framework means that the Band structure applies across Job Functions, Families, etc. In addition, "Bands provide employees with flexibility to move and gain experience. Employees can move across the organization, across product engines, business units or even functions, gaining greater understanding of the business as a whole instead of specializing in one area."[44]

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[39] Ex. 500, Slide 26, "Total Rewards Fundamentals: Managing Pay at Nike."

[40] Walker Dep. 25:9-11.

[41] Walker Dep. 25:23-26:4.

[42] See notes in Ex. 507, Slide 5, "Nike Job Architecture," NIKE_0023548.

[43] See notes in Ex. 507, Slide 5, "Nike Job Architecture," NIKE_0023548.

[44] Ex. 504, "Bands and Levels," NIKE_00002393. Nike's corporate witness, Mr. Walker, testified that this statement was accurate and that "across the organization" meant "across Nike as a full company." (Walker Dep. 29:6-19.)

b. Within the VALUES bands, Nike includes Levels, which are more granular than Bands. The data show there are 11 Levels within Bands L through S with at least two Levels within each Band; the Levels run from "Entry Professional" in Band L to "Senior Director" in Band S.[45] My understanding from deposition testimony is that Levels correspond to a similar level of responsibility across jobs: "[i]t's the framework" used by Nike "so each job within a certain level would have generally the same level of responsibility across those criteria."[46]

c. Within each of these levels, Nike also classifies employees as "Individual Contributors" or "Managers," but these classifications are almost perfectly distinguished by Job Levels.[47]

d. Above the VALUES bands are the "Executive" Bands which Nike labels as E7 to E1, with E7 being the lowest and E1 being the highest.[48] These are "vice president levels."[49] These levels are referred to as E7+.[50] Since the employees in the jobs within E7+ are vice presidents these jobs are not included in the Covered Positions.

37. Nike further uses "Job Codes" and "Job Titles." These are almost always equivalent, with "Job Titles" being recorded as text that captures the Job Level and Job Subfamily (e.g., "PROF SR: EXT COMM" for the External Communications Job Subfamily (which is in the

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[45] See, e.g., Ex. 507, Slide 6, "Nike Job Architecture," NIKE_0023548. According to Nike documents, like Ex. 507, Nike only has 10 Levels; however, the data show 11 Levels (with the addition of Senior Manager), which only appeared prior to the class period.

[46] Walker Dep. 54:2-5.

[47] Managers are almost always in the following Job Levels: Supervisor, Manager, Director, or Senior Director. I estimated some models breaking out the control for manager, and it never had a material impact on the estimates.

[48] Ex. 504, "Bands and Levels," NIKE_00002393., and Ex. 507, Slides 6-7, "Nike Job Architecture," NIKE_0023548.

[49] Matheson Dep. 181:7-13.

[50] Matheson Dep. 181:16.

Communications Job Family in the Communications Job Function), at the Senior Professional Level. The overlap nearly perfectly, but not quite.[51]

38. In addition to estimating models controlling for Job Subfamilies and Job Levels, I also estimate models including the interactions between these. This introduces a separate control for each unique Job Subfamily-Job Level pair in the data. I do this based on the conclusions of Dr. Lundquist, who opines that: "So, according to Nike's job architecture, Subfamily (within Job Function and Job Family) is the most differentiated *Type of Work* variable and Job Level within Band is the most differentiated *Level of Work* variable. Jobs at the same Job Level within Subfamily have been designated by Nike as similar in job content or type of work and require a similar level of knowledge, experience, scope, effort and responsibility based on the leveling criteria. In fact, a 2018 Nike HR document entitled "Total Rewards Deep-Dive" stated as a core belief that, "We believe that all employees, at the same level and in similar jobs with similar performance, should be equitably compensated" (Exhibit 591, NIKE_00001653, p. -1660). Nike uses Job Level to benchmark pay with other similar companies via salary surveys (Exhibit 500, NIKE_00003191, slide 26; Exhibit 507, NIKE_00023548, slide 6; Exhibit 508, NIKE_00024354, slide 7)."[52]

39. The difference between controlling for the interactions, rather than just controlling separately for Job Subfamilies and Job Levels, is that, in the former case, differences in pay across Levels are not restricted to be the same across Job Subfamilies (or, vice versa). In contrast, in the latter case they are restricted in this way.

[51] The file *JobCode History.xlsx* shows some instances when the same Job Code was assigned to different Job Titles. For example, until 4/21/2013 the job code A0017 was associated with the Job Title DIR SR: COMPL & ESH. After 4/21/2013 the Job Code A0017 was associated with the Job Title VP: COMPL & ESH.

[52] Expert Report of Dr. Kathleen Lundquist, p. 21.

a. The table below gives a simple illustration. Assume there is one woman and one man in each Job Subfamily and Level combination, for a total of four women and four men. The pay levels of these eight individuals are reported in the *Job Subfamily-Job Level Pair* cells of the table, with the female shortfalls reported in the last column. In the *Job Subfamilies* and *Levels* rows, each cell contains an average of the pay data for two people. For example, the entry for women for Job Subfamily A is the average of the two numbers in the same column. The table shows that the gender pay gap takes on three different values for the Job Subfamily-Job Level Pairs. Thus, for example, the gender pay gap in Job Level II depends on which Subfamily the person is in. In particular, the gender pay gap in Job Level II is $11,000 in Subfamily A and $12,000 in Subfamily B. But in the bottom panel, the gender pay gap in Job Level II is constrained to be the same regardless of Job Subfamily ($11,500, the average). It is assumed that there is one person of each gender in each *Job Subfamily-Job Level Pair*. The entries for *Job Subfamilies* and *Levels* are then averages across two people.

	Base Pay—Women	Base Pay—Men	Female shortfall within cell
Job Subfamily-Job Level Pairs			
Job Subfamily A/Management Level I	$50,000	$60,000	−$10,000
Job Subfamily A/Management Level II	$55,000	$66,000	−$11,000
Job Subfamily B/Management Level I	$60,000	$70,000	−$10,000
Job Subfamily B/Management Level II	$67,000	$78,000	−$12,000
Job Subfamilies			
Job Subfamily A	$52,500	$63,000	−$10,500
Job Subfamily B	$63,000	$74,000	−$11,000
Levels			
Level I	$55,000	$65,000	−$10,000
Level II	$60,500	$72,000	−$11,500

40. With this in mind, first, I add control for Job Subfamilies (the most disaggregated horizontal classification of jobs),[53] in column 2 of Table 2. When controls are added for Job Subfamilies, in the form of dummy variables for each Job Subfamily, the female dummy variable estimates the gender gap in pay within Job Subfamilies. That is, my estimated gender differences in pay will not reflect the possibility that men and women are in different Job Subfamilies with different levels of pay. Because the resulting gender differences in pay that I estimate are based only on comparisons of men and women in the same Job Subfamilies, labor economists describe such estimates as arising from pay differences "within" Job Subfamilies.[54] There is one constraint imposed, which is that the gender gap is the same across these Job Subfamilies. That is, I estimate a single regression model for pay that allows for differences in pay with worker characteristics (like tenure), and across jobs (like Job Subfamilies), yielding an average gender pay differential, within Job Subfamilies, conditional on all of these controls.

41. With the addition of the Job Subfamily controls, for base pay, the estimated female shortfall is 6.37% (12.06 SDs), implying a shortfall of $8,484 per woman per year. For combined base pay plus bonuses, the estimated female shortfall is 7.35% (12.25 SDs), implying an annual shortfall of $11,363.[55]

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[53] I refer to Nike's Type of Work component of its architecture as "horizonal" classifications or controls and I refer to Nike's Level of Work component of its architecture as "vertical" classifications or controls.

[54] See, e.g.: Groshen, Erica. 1991. "The Structure of the Female/Male Wage Differential: Is It Who You Are, What You Do, or Where You Work?" *Journal of Human Resources*, Vol. 26, pp. 457-472.

[55] It is almost exactly the case that Job Families map uniquely to Job Functions, and Job Subfamilies map uniquely to Job Families, in which case controlling for Job Function or Job Family when I already control for Job Subfamily would have no impact on the estimates, and be redundant. However, there is a handful of deviations from this strictly hierarchical mapping. However, if I add Job Function or Family as separate controls, the estimates are virtually identical.

42. The smaller female pay shortfall when I control for the detailed horizontal classifications of jobs (Job Subfamilies) implies that, on average, women at Nike are employed in somewhat lower-paying Job Subfamilies. It is possible that these control variables actually capture some skill differences between men and women employed at Nike. However, it is also possible that Nike disproportionately hires or moves women into Job Subfamilies (or Job Functions or Families) that are lower paying than those that require similar skills into which men are more likely to be hired or moved.

43. I next also consider the role of vertical differentiation of jobs. In column 3 of Table 2, I retain the Job Subfamily controls, but add controls for Job Levels.[56] When the Level controls are included, the estimated female pay shortfall should be interpreted as comparing women's and men's pay for women and men in the same Job Subfamily and Level, rather than just in the same Job Subfamily. Thus, for example, if women are on average in lower-paying Job Levels than men, the estimated female shortfall should decline, because the Job Level controls "remove" the influence of women being employed in lower-paying Levels. Whether the estimate with or without the Job Level controls provides a better estimate of gender discrimination in pay depends on whether discrimination plays role in the assignment of women to lower-paying Job Levels (a phenomenon I document just below, and in more detail later in this report).

[56] Although Nike documentation (Ex. 500, Slide 26, "Total Rewards Fundamentals: Managing Pay at Nike") suggests that Job Level completely subsumes Band, the Band/Job Level Hierarchy is not strict, based on the data. There are examples of one Job Level being associated with multiple Bands. One such example would be the Expert Professional Job Level associated with Bands U, E, and S. However, if I add Job Band as well as Job Level controls, the estimates are virtually identical.

44. Adding the Job Level controls, the female base pay shortfall is 1.76% (8.35 SDs), implying a shortfall of $2,344 per woman, per year. The gap is 2.01% (9.00 SDs) for base pay plus bonuses, implying an annual shortfall of $3,107.

45. The fairly substantial reduction in the female pay shortfall when adding controls for Job Levels – although remaining sizable and strongly statistically significant – implies that women are employed in lower-paying Job Levels within Job Subfamilies. This could happen for one of two reasons (or both) – hiring of women into lower Job Levels, and faster promotion of men to higher Job Levels. I show later that both of these occur.

46. In column 4 of Table 2, I add controls for all interactions between Job Subfamilies and Job Levels, or, equivalently, dummy variables for each unique combination of Job Subfamily and Job Level that appears in the data. Adding these more-detailed controls has virtually no impact on the estimates. The female base pay shortfall is 1.78% (8.97 SDs), and 2.03% (9.55 SDs) for base pay plus bonuses. The implied pay shortfalls, per woman per year, are $2,371 and $3,138, respectively.

47. One key takeaway from Table 2 is that a female pay shortfall persists even when we compare women and men in the same Job Subfamily and Job Level, with the same age, job tenure, time in job, and performance. This female pay shortfall is consistent with pay discrimination against women doing substantially similar work and with similar skills, qualifications, and performance to men.

48. The second takeaway is that the female pay shortfall is substantially larger (more than three times as large) when the controls for vertical differentiation of jobs – i.e., Job Level – are excluded. This implies that, within Job Subfamilies, women are employed in Job Levels that

pay a good deal less. Later in my report I discuss evidence on why women are in lower-paying Job Levels.

49. Next, I report my key analyses for specific subperiods that I understand may correspond to different claims (see para. 17). I do this for the estimates corresponding to columns 2, 3, and 4 in Table 2. Specifically, columns 1-6 of Table 4 report results restricting the analysis period to begin in 2016 and then in 2017 (instead of 2015, in Table 2).

50. As shown in columns 1-3 of Table 4, when the analysis period begins in 2016, for the estimates including Job Subfamily controls, the female base pay shortfall is 6.24% (11.68 SDs, $8,384 per woman per year), and 7.13% (11.84 SDs, $10,955 per woman per year) for base pay plus bonuses (column 1). Adding the Job Level controls (in column 2), the female base pay shortfall is 1.63% (7.67 SDs, $2,190), and 1.86% (8.27 SDs, $2,858 per woman per year) for base pay plus bonuses. And when I add controls for the Job Subfamily and Job Level interactions, in column 3, the female base pay shortfall is 1.68% (8.38 SDs, $2,257 per woman per year), and 1.91% (8.92 SDs, $2,935 per woman per year) for base pay plus bonuses. The estimates in column 3, adding the Job Subfamily and Job Level interactions, are very similar to those in column 2, but slightly larger.

51. As shown in columns 4-6 of Table 4, when the analysis period begins in 2017, for the estimates including Job Subfamily controls, the female base pay shortfall is 5.95% (10.87 SDs, $8,063 per woman per year), and 6.83% (11.04 SDs, $10,596 per woman per year) for base pay plus bonuses (column 4). Adding the Job Level controls (in column 5), the female base pay shortfall is 1.53% (7.03 SDs, $2,073 per woman per year), and 1.80% (7.78 SDs, $2,793 per woman per year) for base pay plus bonuses. And when I instead control for Job

Subfamily and Job Level interactions, in column 6, the female pay shortfalls are slightly larger.

52. Columns 7-10 of Table 4 present results for those hired before September 2017, and those hired September 2017 or later, for the specification first without and then with interactions of Job Subfamily and Job Level. The relevance of this analysis is discussed in more detail below, when I study starting pay. Briefly, however, Nike claims to have stopped using prior pay to set starting pay in September 2017.[57] By focusing on these two subperiods, *based on hire data*, we can see whether it is possible that differences in prior pay policies are reflected in class period pay. The estimates in columns 9 and 10 suggest they are, as the estimate for those hired up to September 2017 is negative and significant (a 1.89% female pay shortfall, 8.88 SDs, $2,517 per woman per year), while the estimate for those hired in September 2017 or after is much closer to zero (0.21%, $280 per woman per year) and not statistically significant. This suggests that prior pay helped determine starting pay, generating a female

[57] A Nike document, "TA Comp History Policy" (Ex. 513, from August 2017) directed that Nike employees may no longer "ask candidates or their employers questions about their compensation history," "use salary as a tool for disqualification or as a screening criteria," "search public records for candidates' compensation history," or "record salary history information in the ATS/CRM." (ATS and CRM refer to systems that Nike uses to collect personnel data; Walker Dep. 97:14-17); "TA" stands for Talent Acquisition and it is part of Nike's HR organization (Walker Dep. 95:6-9). As further evidence of the change in prior pay policy, an October 12, 2017, email from Daniel Laboe, "Sr. Director Talent Acquisition Operations & Innovation," to talent acquisition employees, instructs that Nike "will no longer ask candidates about their compensation history." (Ex. 673.)

Other evidence indicates this was a policy change. Shine Thomas says this in her deposition testimony (Thomas Dep. 214:20-215:4). Shane Walker, in his deposition, was asked "Has Nike eliminated the collection of candidate salary history?" He responded "Yes, in conjunction with the change in Oregon law that required we could no longer use that as one of the data points when making or passing an offer for a candidate." (Walker Dep. 93:22-94:2.) I am informed that the change in Oregon law regarding the use of prior salary in establishing compensation became effective in October 2017.

Other Nike documents acknowledge that Nike had used prior salary history in setting compensation. A Nike document states that "[i]n the past, we often focused on the new hires prior salary and/or the size of the increase the new hire would receive." (Ex. 515, Slide 6, FY20 Total Rewards Update.) Nike's Chief Human Resources Officer, Monique Matheson, referenced this prior policy related to the collection of prior salary history, in an email dated April 4, 2018, to all Nike employees stating that Nike "remove[d] bias from critical moments of the hiring process-- by… eliminating the collection of candidate salary history …" (Ex. 512, NIKE_00002235.)

pay shortfall in starting pay that persists into the class period. This is consistent with evidence on starting pay I discuss later.

53. Columns 11 and 12 of Table 4 show these estimates for the 2018-2019 period, for those hired before September 2017. The estimates indicate female pay shortfalls similar to those for the whole period. What this indicates is that even if starting pay gaps for women became smaller (and statistically insignificant) for those hired in September 2017 or later, pay gaps for women *employed* in that period, but hired earlier, remained sizable and statistically significant. This is true both with and without controlling for Job Levels.

54. Finally, Table 5 presents estimates of the same specifications, but separately by year. I show these estimates for the specification with the controls for Job Subfamily, and then with the interactions between Job Subfamily and Job Level – the same specifications as the subperiod analyses in columns 1, 3, 4, and 6 of Table 4. I do not describe all of the estimates in the text, but there are two conclusions. Virtually every estimated gender pay gap is negative – i.e., women are paid less – and statistically significantly so, even when I disaggregate the estimates by year (only one estimate is not statistically significant at the 5% level – for bonuses only, in column 4).

VII. Starting Pay

55. I next turn to an analysis of starting pay. I do this analysis for those Nike employees employed during the class period for whom I have starting pay. It is my understanding that Plaintiffs' counsel requested the starting pay for employees who worked in a Covered Position during the class period but Nike only provided the information for those hired between January 1, 2012 and September 1, 2019. I have data on 6,713 employees with starting pay data; there are 12,503 employees in the data for whom I can estimate models of

pay during the class period. All else the same, the smaller number of observations with starting pay would reduce the standard deviations and statistical significance of any gender disparity I estimate.[58]

56. I begin, in column 1 of Table 6, with a regression model that includes only the female dummy variable and year dummy variables and the individual controls from earlier that are relevant for a new employee. Note that in these models, unlike Table 2, I do not have controls for job tenure or performance, since the models are estimated for pay when one starts at Nike. By the same token, the analysis of starting pay is done only for base pay. As I explained earlier, age is used as a proxy for experience, and I do not have a standard experience measure in the data. However, as discussed later, for a large part (but not all) of the starting pay sample, I can match records to job history and hence construct explicit experience measures, precluding the need for the crude age control. Thus, although Table 6 shows results with the age control (for the larger sample), these are not my preferred starting pay estimates.

57. For this model, column 1 shows that the estimated female pay shortfall is 7.54% (9.86 SDs), $8,756 per woman.

58. Next, as I did with the analysis of pay in the class period, I add controls for the structure of jobs at Nike – first adding controls for Job Subfamilies and then adding controls for Job Levels, and finally the interactions between these. Since these are not my main starting pay estimates, I discuss them briefly. With horizontal (Job Subfamily) controls, in column 2 of Table 6, the female pay shortfall in starting pay is 2.08% (3.00 SDs, $2,416 per woman).

[58] To be more precise, the SDs of the estimated female shortfall in pay (barring any other changes), is approximately proportional to the square root of the sample size. So for any SD calculation using this smaller sample, a rough guide is that with the full data the SDs would be larger by 36.5% (or a factor of 1.365 = square root (12,503/6,713); i.e., the results would be more strongly statistically significant.

With the vertical (Job Level) controls as well, the female starting pay shortfall is 0.79% (2.34 SDs, $917 per woman). With the Job Subfamily and Job Level interactions, the female starting pay shortfall is 0.95% (2.90 SDs, $1,103 per woman).

59. Finally, columns 5 and 6 of Table 6 show results before September 2017, and for September 2017 and after, for the last (most-detailed) model. (Since the data in this table are for starting pay, these are hire dates.) This analysis is motivated by Nike's assertion that it stopped using prior pay to help determine starting pay in September 2017, as discussed earlier (footnote 56). If prior pay reflects gender discrimination in pay in the labor market in general, then basing starting pay in part on prior pay would be expected to replicate that general labor market discrimination in starting pay at Nike. The estimates in columns 5 and 6 of Table 6 are consistent with that hypothesis, as the estimated female starting pay shortfall is 1.06% (2.39 SDs, $1,231 per woman) in the period before September 2017, but falls by more than half (to 0.48%) and is not statistically significantly different from zero (1.00 SDs, a shortfall of $557 per woman), for the period September 2017 and after.

60. For a large subset of those observations with starting pay information (4,902 out of 6,712 observations, or 73%), I am able to attach information on education and prior job experience from job applications. With these data, I can construct detailed controls for education and prior work experience, to estimate the gender gap in starting pay for similar women and men hired into similar jobs, with more-detailed controls for differences among workers. To start, I re-estimate the models from columns 2 and 4 of Table 6, for the sample I can match to applications. Comparing columns 1 and 2 in Table 7 to those columns, we see that the estimates are close, indicating that the subsample with matched applications data is representative of the large sample of starting pay observations. (This is demonstrated further

in Table B3, which shows similar descriptive statistics for the matched and unmatched observations.) However, as noted above, I am less interested in the specifications including age controls, since – as I describe next – I can instead construct detailed prior experience controls for the matched applications.

61. For the education information, I add controls for the highest educational degree achieved, and I incorporate information on the rankings of the college or university from which the highest degree was obtained.[59] The methods I use to do this are explained in Appendix C. I thus control for the amount of education and the quality of the educational institution.

62. For prior work experience, I use statistical methods coupled with machine learning to group the prior jobs new Nike employees held into similar categories, and then measure the amount of time in each of those "clusters" of jobs (and the square of that time). The methods I use to do this are explained in Appendix D. This method differs from the approach of simply defining variables for a large number of prior jobs held based on the exact text used to describe those prior jobs. One issue with the latter approach is how many such prior job types to define (throwing the remainder into a "catch-all" category of job descriptions that appear in the data with very low frequency). The other problem is that if one tries to define a very large set of variables for different prior job titles, the estimated gender gap can become unreliable; the large number of different prior job titles may not capture any variation in the relevance of prior experience, but because they may be correlated with being female, can absorb variation in pay that is properly attributable to gender. My approach limits the

[59] Note that this is richer information than what is typically done in many labor economics studies of the effects of schooling on wages, which use either years of schooling or dummy variables for highest degree earned (depending on what is available in the data set used). Information on the quality/ranking of the college or university likely conveys some additional information on the abilities of the student. Brewer, Dominic J., Eric R. Eide, and Ronald G. Ehrenberg. 1999. "Does It Pay to Attend an Elite Private College? Cross-Cohort Evidence on the Effects of College Type on Earnings." *Journal of Human Resources*, Vol. 34, No. 1, pp. 104-123.

number of prior job titles used, while clustering these job titles into related jobs. And it puts all prior job titles in a cluster based on similarity, rather than leaving a huge category of prior job titles that get lumped together despite having no similarity. However, I show that the results are consistent using either approach.

63. Columns 3 and 4 of Table 7 present my estimates incorporating this education and prior work experience information. In column 3, with the controls for Job Subfamily, the estimated female starting pay shortfall is 3.32% (3.84 SDs, $3,852 per woman). When I instead include the Job Subfamily and Job Level interactions, hence capturing the vertical categorization of jobs as well, this gap is 1.18% (3.31 SDs, $1,368 per woman). Note that the female pay shortfall is no smaller – and in fact somewhat larger – when I add these education and prior experience controls. For example, compare the 1.09% shortfall in column 2 to the 1.18 shortfall in column 4. This implies that, on balance, education and prior experience do not contribute at all to lower starting pay of women, and instead that women have more of the education and types of prior experience that is valued by Nike.[60]

64. Finally, columns 5 and 6 of Table 7 again split the sample into the periods when, according to Nike, it was and was not using prior pay to set starting pay. In the period when they were using prior pay (before September 2017), the estimated female starting pay shortfall was 1.33% (2.70 SDs, $1,540 per woman). It is about half that in the period beginning in September 2017, and no longer statistically significant at the 5% level (1.00 SDs, $649 per woman).

[60] Table B4 (in Appendix B) shows that men have slightly higher (total) prior experience, and that while a slightly larger percentage of men have advanced degrees, the percentage of women with Bachelor's degrees is more than 10 percentage points higher, and there are also higher percentages of men whose highest degrees are below a Bachelor's.

65. Note that the overall starting pay gap for those hired before September 2017, of 1.33%, (Table 7) is 75% of the class period pay gap of 1.78% (Table 2), for the most closely corresponding specifications. This evidence implies that women are paid less than men who start in the same jobs at Nike, and that this starting pay gap persists into the class period and in fact becomes somewhat larger.

66. The machine learning procedure I use to cluster jobs has parallels to other research I have done.[61] In Table B5, in Appendix B, I show that I obtain quite similar results if I instead just use a large number of indicators for the most common (118) colleges and universities and a residual category, and I classify jobs into those that are most common (252 prior job titles) and then a residual category, and then define experience (and its square) in each of those jobs. The estimates in columns 1 and 2 of Table B5 correspond to those in columns 3 and 4 of Table 7, and indicate a similar pattern of female shortfalls in starting pay.

VIII. Class Period Pay Regressions with the Applications Data Sample and Controls

67. The preceding paragraphs and Table 7 have focused on the estimates of starting pay regressions for the subsample of observations for which I can match application data and hence include detailed education and prior experience controls. I have drawn inferences from these estimates about the female pay shortfall in starting pay and how it is reflected in the female pay shortfall in pay during the class period discussed earlier in my report (e.g., Table 2). However, the two sets of estimates are not directly comparable because the class period pay estimates are based on the full set of observations, for which some of the detailed controls available from the matched application data are not available. Moreover, it is useful

[61] Burn, Ian, Patrick Button, Luis Felipe Munguia Corella, and David Neumark. "Does Ageist Language in Job Ads Predict Age Discrimination in Hiring?" Forthcoming in *Journal of Labor Economics*.

to know if the more-detailed control variables available from the applications have any impact on the estimated female pay shortfall in the class period. For example, with this smaller sample I can construct a detailed prior experience measure, and measure education. It is therefore of interest to explore the evidence on the gender pay gap in class period pay for the subset of observations for which I have these matched application data, using the controls from the application data. This allows me to compare estimated pay regressions more directly for the class period and for starting pay, using the same controls.

68. This analysis is reported in Table 8. Here, I estimate the class period pay regression for the subset of observations with matched application data. The estimates for the full class period are reported in column 1 of Table 8. Note that here there are 14,563 observations, compared with around 43,500 in Table 2 (Panel A). As already explained, the sample size difference reflects the fact that the applications data that Nike provided only goes back to hires beginning in 2012, and that I cannot match all hires to applications data. Table 8 reports this analysis for base pay, which provides the best comparison with the starting pay results that motivate this analysis.

69. In column 1 and 2 of Table 8, I report the analysis without including the controls for vertical classifications of jobs, but instead just include the Job Subfamily controls. In columns 3 and 4 of Table 8, I instead include the Job Subfamily and Job Level interactions.

70. Turning first to column 1 of Table 8, I keep the specification the same as in column 2 of Table 2, but just use the smaller matched sample. The estimated female pay shortfall in column 1 of Table 8 is 3.51% (4.39 SDs), or $4,348 per woman per year. Note that this is more than one-third smaller than the comparable full sample estimate in column 2 of Table 2, which was a 6.37% female pay shortfall (12.06 SDs, $8,484 per woman per year). This

simply says that for the subset of observations for which I have application data, the class period pay gap is a little bit smaller.

71. In column 2 of Table 8, I add the detailed education and prior experience controls that I added to the starting pay regressions in Table 7. Note that, unlike in Table 2, I no longer include age and its square. That is because once I control for experience prior to coming to Nike, plus tenure at Nike, I have controlled for the total amount of work experience. The estimate in column 2 of Table 8 is larger than in column 1 (a 4.52% female pay shortfall, 5.16 SDs, $5,600 per woman per year).

72. Columns 3 and 4 of Table 8 report the same models but substituting the Job Subfamily and Job Level interactions, which incorporate variation in job levels within Job Subfamilies. In column 3, I keep the specification the same, but just use the smaller matched sample. The estimated female pay shortfall in column 3 is 1.10% (3.50 SDs, $1,363 per woman per year). Note that this is about one-third smaller than the full sample estimate with fewer controls in column 4 of Table 2, which was a 1.78% female pay shortfall (8.97 SDs, $2,371 per woman per year). Again, this simply says that for the subset of observations for which I have application data, the class period pay gap is a little bit smaller.

73. In column 4 of Table 8, I add the detailed education and prior experience controls that I added to the starting pay regressions in Table 7. Again, unlike in Table 2, I no longer include age and its square. That is because once I control for experience prior to coming to Nike, plus tenure at Nike, I have controlled for the total amount of work experience. The estimate in column 4 is nearly identical to column 3 (a 1.03% female pay shortfall, 3.29 SDs, $1,278 per woman per year).

74. Finally, columns 5 and 6 of Table 8 show, as before, that there is a statistically significant female pay shortfall for those hired before September 2017 (1.07%, 2.83 SDs, $1,322 per woman per year), which falls to half that, and not significant at the 5% level, for those hired in September 2017 or later.

75. The key conclusion from this analysis is that, once I use a comparable sample and set of control variables, the class period female pay shortfalls for comparable women and men doing substantially similar work are fairly close to the starting female pay shortfall from comparable women and men hired into substantially similar jobs.

a. Referring back to Table 7, for the model with the Job Subfamily controls (column 3), the female shortfall in starting pay is 3.32% (3.84 SDs), very similar to the class period estimate of 4.52% (5.16 SDs) in Table 8 (column 2).

b. The same is true when I instead include the Job Subfamily and Job Level controls. The female shortfall in starting pay for Table 7 (column 4) is 1.18% (3.31 SDs), very similar to the class period estimate for Table 8 (column 4) of 1.03% (3.29 SDs).

76. Another conclusion from Table 8 is that the estimated female pay shortfalls are larger in column (2) than in column (1). These columns are estimated for the same sample, with the only difference being that column (2) adds the more-detailed education and experience controls. Thus, the fact that the estimated female pay shortfalls grow when these controls are added implies that differences in education and prior work experience do not help explain why women are hired into lower Job Levels and Nike and are paid less at Nike. Indeed, it is the opposite. With the more-detailed controls, the estimates imply a stronger allocation of women to lower-paying job levels.

a. In contrast, the female pay shortfalls are very similar in columns 3 and 4 of Table 8.

The latter result indicates that differences in prior qualifications of women and men do not explain the gender gap in pay within Job Subfamilies and Levels.

IX. Merit Increases

77. I began my analysis by documenting the female pay shortfall at Nike during the class period. I then showed how the data indicated that prior pay policies generated a similar female pay shortfall in starting pay. If women who are comparable to men are hired at lower pay (as I have shown), then even if women and men receive comparable percentage pay increases once they are at Nike,[62] then women would be disadvantaged at each merit pay increase, because the same percentage increase on a smaller base amount is a smaller dollar increase.

78. To provide evidence on merit pay increases, Table 9 reports estimates of models for merit pay increases that use exactly the same sets of controls as the corresponding columns of the class period pay regressions.[63] However, since this analysis concerns a Nike practice that has

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[62] There is certainly no reason to expect women to receive lower percent increases, given that – as shown in the lower panel of Table 3 – they receive better performance ratings. From 2013 to the present, Nike provided three types of guidance for the award of merit increases. During this entire period, CFE performance ratings were relevant to the award of merit. (Walker Dep. 360:13-25.)

First, Nike's guidelines provided for a "minimum and maximum" for each CFE rating. (Walker Dep. 360:13-25.) "For example, … for an employee rated successful, the guideline that would have shown could have been 3 percent to 5 percent. For an employee that was highly successful, the guideline could have shown 4 percent to 6 percent." (Walker Dep. 366:6-367:3.)

In 2018, FY19, Nike instituted guidance "in a slightly different way" for merit increases "with a focus on how do you position someone in the pay range." (Walker Dep. 313:11-17.) Under this new guidance an employee who was positioned lower in a pay range, such as in the lower third, "would receive a larger increase" than an employee who was positioned higher in the pay range, such as in the upper third (Walker Dep. 327:6-17; "Assessing Performance Summary," Ex. 538, NIKE_C_00001653 at -1666).

In 2019, FY20, Nike instituted the Annual Pay Review (APR). (Ex. 523, "Approximate Timing of Compensation Process & Program Changes.") Under APR, "core pay was Nike's new term for merit. (Ex. 522, at p. 34, Annual Pay Review Deep Dive.) Pursuant to APR, core pay increases were based upon four categories: "Max Invest," "Invest," "Market," and "Zero." (Ex. 522 at pp. 34-35.) Market increases were "prepopulated" by a centralized process based upon an employee's country and position in a pay range. (Ex. 522 at p. 35.) For approximately 20% of direct reports a manager could recommend a further increase, Max Invest or Invest, or a decrease, "Zero." (Ex. 522 at pp. 34-35.) As a Nike document states, prior to APR's implementation, "[b]ase pay increases were driven by the CFE rating." (Ex. 530, NIKE_C_ 0003279 at -3281.) While "CFE ratings no longer drive the default Core Pay increase, … performance continues to be an important factor …." (Ex. 530, at -3281, "Annual Pay Review.")

an ongoing impact on base pay, I report results for the full 2013-2019 period, as I did for the starting pay analysis. (Note that because I need controls from the previous year, as explained below, and the first year of data cover 2012, the analysis begins in 2013.) It is, admittedly, not usual to estimate these kinds of models for raises as opposed to levels as pay, but since both are pay measures, theory implies they should be driven by the same variables, so these regression models can be justified by the same labor economics research that justifies the earlier pay regression models.

79. I measure merit increases two ways. First, I use the dollar amount (as always, corrected for inflation). Second, I use the log of the merit increase amount.[64] In this case (using the log of the increase), the measure captures, approximately, the percentage difference between the dollar value of the merit increases for men and women.

80. My understanding is that merit increases are decided at the end of the fiscal year, which is May 31, and are tied to the performance ratings as of that time.[65] For this reason, in my analysis of merit pay increases, I "date" the control variables to the prior calendar year (denoted "t−1" in the tables).

81. In Table 9, column 1 includes the Job Subfamily controls, and column 2 adds the interactions between Job Subfamily and Job Level. In column 1, the estimate in Panel A indicates lower merit pay increases for women – by $235 (8.85 SDs). In Panel B the shortfall is 2.96% (1.62 SDs). When the interactions with Job Level are added, in column 2,

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[64] I substitute $1 for merit increases of zero, because the log of zero is undefined, but a $1 entry will still capture the essence of essentially no merit increase.

[65] See the calendar in Ex. 614, "Performance Rewards FY16," NIKE_00030252. Although this calendar refers to 2016, Treasure Heinle, in her deposition, testified that could not think of any other changes to the calendar until 2020, when there were no merit increases (because of COVID-19), and which is beyond the range of the data I analyze.

the estimated dollar differences (Panel A) are small and statistically insignificant, while women get larger log (percentage) increases (Panel B).

82. The implication of these estimates, and in particular the differences between columns 1 and 2, is that women are disadvantaged by two things at Nike – the employment of women in lower-paying Job Levels, and the use of equal percent increases for similar performers. Women at Nike are employed in lower-paying Job Levels – which, as indicated earlier and shown more explicitly below – stems from the Job Levels into which they are hired. (And, as I show below, women's employment in lower-paying Job Levels is exacerbated by their lower promotion rates relative to similarly qualified men.) Nike bases merit pay increases on a percentage of base salary, linking CFE ratings to a range of percentage increases. Thus, when equally performing women get similar percentage increases to men, because these increases are calculated relative to their base pay, their merit increases are smaller. Within Job Levels (as indicated in column 2), this disadvantage is no longer present. But across Job Levels (column 1), it is. The key point is that the initial Job Level placement, coupled with how merit increases are calculated, essentially locks in the pay disadvantage of women at Nike.

X. Bonuses

83. My earlier tables on the gender gap on class period pay (Tables 2-5) reported separate results for bonuses, although I focused on the results for base pay, and for base pay plus bonuses combined. I have been asked to discuss the results for bonuses separately because the awarding of bonuses can be viewed as a distinct pay practice. Bonuses at Nike are referred to as the Performance Sharing Plan ("PSP"), which is an annual cash bonus

described as a "short-term incentive" program.[66] The bonus award is based upon a percentage calculation of base pay.[67] As is the situation with merit increases, since Nike's practices generated a female shortfall in base pay, then even if women and men receive comparable percentage bonus awards, women will be disadvantaged in the dollar amount of the bonus awards.

84. I begin with the core class period pay analysis in Table 2. The results for bonuses are reported in Panel B. In column 1 of Table 2, I estimate the model with the worker controls I have that are suggested by standard labor economics, and available in the data – age (as a proxy for experience) and its square, tenure at Nike (and its square), time in job (and its square), and performance ratings. As column 1 shows, there is a sizable female pay shortfall in bonuses (19.41%), which is strongly significant (15.49 SDs), representing a shortfall of $3,923 per woman per year. The implication is that the lower bonuses for women documented thus far are not explained by women receiving lower performance ratings or having been at Nike for less time (lower tenure) or in their specific job at Nike for less time.

85. Next, I add controls for Job Subfamilies, which describe the horizontal classification of jobs at Nike. The female pay shortfall in bonuses is 12.45% (11.18 SDs, $2,516 per woman per year).

86. Columns 3 and 4 of Table 2 add the Job Level Controls, first separately, and then interacted with Job Subfamily. The estimates are similar, indicating a female pay shortfall of about 3.16% (5.5 to 5.7 SDs, and about $640).

66 Ex. 500, Slide 19, "Total Rewards Fundamentals: Managing Pay at Nike."

67 White Dep. 123:22-124:6, and "FY16 PSP 2-Factor Payout Examples" NIKE_00030282, April 8, 2016.

87. Note that for this specification – as well as for all of the specifications in Table 2, the female pay gap is larger for bonuses than for base bay, in relative (i.e., percentage) terms. For example, in column 2, the female pay shortfall in base pay is 6.37% (representing $8,484), while the female pay shortfall in bonuses is 12.45% ($2,516). The same is true when I control for Job Levels in columns 3 and 4. Finally, as already discussed earlier, with regard to Table 2, I always find strongly statistically significant female pay shortfalls for combined base pay plus bonuses.

88. Finally, note that in Table 4, when I focus on those hired September 2017 or later (columns 8 and 10), I did not find a significant female pay shortfall for bonuses. This is consistent with two facts: (i) as Table 4 also shows, the gender base pay gap for those hired in this subperiod is small and statistically insignificant; and (ii) as noted earlier, Nike bases bonuses on base pay (as a percentage), so the smaller female pay shortfall in base pay is reflected in the smaller female pay shortfall in bonuses.

XI. Promotions

89. I next turn to an analysis of promotions. It is useful to first reiterate what we have learned from the analysis of pay that is potentially relevant to promotions. The clearest starting point comes from looking at the estimates for starting pay, in Table 7, and for class period pay for the same individuals included in the starting pay analysis, in Table 8.

90. For base pay when starting, in Table 7, the estimated female pay shortfall with the detailed controls for Job Subfamily is 3.32% (3.84 SDs), and the estimated female pay shortfall with the Job Subfamily and Job Level interactions is 1.18% (3.31 SDs). For base pay, in Table 8, the estimated female pay shortfall with the Job Subfamily controls is 4.52% (5.16 SDs), and

the estimated female pay shortfall with the Job Subfamily and Job Level interactions is 1.03% (3.29 SDs).

91. The widening of the pay gap from starting pay to class period pay, in the regressions that do not control for Job Levels, implies that men move into higher-paying Job Levels at a faster pace than women do, which is why the gender pay gap not controlling for Job Levels increases over time (from starting pay to the class period).

92. Nike defines promotions as an employee moving to "a higher level job (as defined by NIKE's job banding and leveling criteria)."[68] Nike determines whether promotions will be competitive or noncompetitive.[69] Nike has an annual Talent Planning Cadence, where Nike makes decisions regarding whether to fill an open position competitively or non-competitively.[70] The determination of whether a job will be filled through a competitive process or talent planned is referred to as "fill strategy."[71]

93. If Nike determines that a position will be filled competitively, there is a job posting, applications from candidates, and interviews; non-competitive promotions do not have job

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[68] Ex. 550, "Job Changes."

[69] Heinle Dep. 132:1-14 (for positions that open up or will open up at Nike WHQ, there is a discussion on how to approach the fill strategy for a job); Heinle Dep. 130:11-16 (promotions at Nike can be filled in "one of two ways," competitively or non-competitively); Nike also refers to non-competitive promotions as "talent planned" promotions." (Thomas Dep. 67:8-13.)

[70] Heinle Dep. 200:19-201:2. The annual Talent Planning Cadence includes CFE ratings, Individual Development Plans, Potential Appraisal, and Workforce Planning. (Heinle Dep. 177:8-179:6, and Ex. 647, Slide 16.) During Workforce Planning, Nike seeks to have "consistency in position management," which includes guidelines to update the fill strategy for open positions. (Ex. 643 at NIKE_C_00001863; also Ex. 643 at NIKE_C_00001858 (showing screenshot of Nike's "Centralized Data Source for HR and Finance to consistently manage and plan for workforce costs" that includes determinations regarding Fill Strategy, many of which are "non-competitive"); and Ex. 648 at NIKE_00001850 (instructions on how to change an open position to a "planned open position" as part of workforce planning).) An objective of Workforce planning is to "[i]nform 12-18 month talent plans to proactively source and develop talent." (Ex. 648 at NIKE_00001849.)

[71] Heinle Dep. 124:13-125:2. Nike has guidelines regarding fill strategy (see Heinle Dep. 133:6-11), but I am informed that Nike has not produced these guidelines.

postings, applications, or interviews, but are subject to an approval process, involvement of HR, and procedures.[72]

94. Non-competitive promotions are also made during re-organizations, where Nike uses "a consistent, thoughtful and practical approach to making talent decisions" and Nike will "apply a systematic talent management approach."[73] Nike maps employees to organization structure during re-organizations.[74] During re-organizations, business leaders and Nike HR (Talent Management and HRBPs) are involved in finalizing organizational structure, then mapping talent into the structure, and competitive openings are identified.[75,76]

95. To provide additional direct evidence, I estimate models for promotions. I define the outcome as whether one moved up at least one job level from one year to the next. I estimate these models as linear probability models, using the same controls as in my class period pay regressions. I also estimate these for competitive and non-competitive promotions separately.[77]

96. The results are reported in Table 10. Looking at all promotions, in Panel A, before we include the controls for Job Levels women are promoted at a higher rate (columns 1 and 2).

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[72] Heinle Dep. 45:2-6, 73:14-17, 74:8-11, 62:9-63:14; Ex. 636, "Non-Competitive Job Changes Manager Education Tool," NIKE_00003255.

[73] Ex. 707, Slide 2. As with the annual Talent Planning Cadence, HR is involved, including HRBPs (Human Resources Business Partners), and Talent Planning (aka Talent Management) makes decisions "around people and structure." (Ex. 707, Slides 3 and 13; Vales Dep. 115:17-117:5 (HRBPs, Talent Management, and Organizational Effectiveness involved in 2017 re-organization, including talent mapping, with HRBPs involved throughout).

[74] Ex. 707, Slide 5; Vales Dep. 114:12-19.

[75] Vales Dep. 127:9-129:18; also Ex. 711 at NIKE_00038709 (calendar for 2020 re-organization showing that structure is finalized and talent mapping conducted during Leadership Team meeting and then competitive positions are identified to recruit).

[76] Many non-competitive promotions were made during re-organizations. Two large reorganizations at Nike occurred around August 2017 and June 2020. (Vales Dep. 101:1-102:10.) I have data covering the 2017 reorganization, and as shown in Figure B1, there was a spike in non-competitive promotions associated with this reorganization (appearing in the September reading, which covers August).

[77] See footnote 6.

However, once I include the Job Level interactions with Job Subfamily, women are promoted at a lower rate. In column 3 of Table 10, controlling for the interactions of Job Subfamily and Job Level, the promotion rate for women is lower by 0.4 percentage point, although the difference is not statistically significant at the 5% level (1.31 SDs).[78] Relative to the baseline promotion rate, this difference implies a 2.89% lower rate of promotion for women. In other words, once we look at women and men with similar qualifications who are in similar jobs, women are promoted at a lower rate.

97. The estimates in Panels B and C of Table 10 show that this female shortfall in promotions is completely driven by non-competitive promotions. For non-competitive promotions, controlling for Subfamily and Job Level, the promotion rate for women is lower by 0.72 percentage point (2.50 SDs). Relative to the baseline promotion rate, this difference implies a 6.54% lower rate of promotion for women with similar qualifications and doing similar jobs as compared to men.

98. Columns 4 and 5 of Table 10 repeat the model from column 3, but now for the period before April 2018, and for April 2018 and after.[79] These results indicate that the female promotion shortfall for non-competitive promotions is evident in the period prior to April 2018, but not after. In the earlier period, the non-competitive promotion rate for women is lower by 0.87

[78] Comparing promotion rates without controlling for Levels generates misleading or spurious evidence that women are promoted at higher rates. The reason is that promotion rates are highest at some of the lowest Job Levels, and it is those same job levels in which women are more concentrated. One can see this in Table B6. For example, looking at Panel A in Table B6, the promotion rates are generally highest in the two lowest Job Levels (Entry Professional and Intermediate Professional), and women are over-represented in these Job Levels (e.g., 17.4% of women are Intermediate Professionals, vs. 11.0% of men).

[79] I was asked by Plaintiffs' counsel to apply the model before and after April 2018, which I understand is based on an email sent to Nike employees, April 4, 2018, Ex. 512, NIKE_00002233, by Ms. Matheson, Nike's Chief Human Resources Officer, which admitted that "we need to improve representation of women…. While we've spoken about this many times, and tried different ways to achieve change, we have failed to gain traction…," and she described a general plan to increase the representation of women at all Job Levels.

percentage point (2.61 SDs); relative to the baseline promotion rate, this difference implies a 7.90% lower rate of promotion for women with similar qualifications and doing similar jobs as compared to men. From April 2018 and after, the sign reverses, although the gender difference is not close to statistically significant at the 5% level (0.34 SDs).

99. Many promotions are over more than one job level, as documented in Table B7.[80] As a consequence, it is possible that the female disadvantage in promotions could be worse than indicated by Table 10, if we consider regression models like those in Table 10, but estimated for promotions of more than one level.

100. To explore this, Table 11 presents models for the probability of being promoted by two or more, three or more, or four or more levels from one year to the next. Looking at all three columns, we find very robust evidence of lower "multi-level" promotion rates for women. In Panel A, women have a statistically significantly lower probability of promotion across any of these ranges. In Panels B and C, for competitive and non-competitive promotions respectively, there is statistically significant evidence of a lower probability of non-competitive promotions of women across two or more levels or three or more levels. (For four or more levels, the estimate is in the same direction, but is not significant at the 5% level (1.32 SDs).)

101. Overall, then, the analysis of promotions indicates that women with similar qualifications and performance ratings to men and with similar jobs to men are promoted at lower rates. Since higher job levels are associated with higher pay, this gender difference in promotions

[80] If all promotions were one level up, then all of the entries in Table B7 would be in the shaded cells. Observations to the right of these cells, in each row, indicate promotions by two or more levels.

– to the disadvantage of women – can help explain why the gender gap in pay in the class period grows relative to the gender gap in starting pay.

XII. Channeling

102. Nike hires women disproportionately into Covered Positions at lower Job Levels than men, despite women if anything having advantages in education and prior experience.

103. The hiring of women into lower Job Levels is apparent from the starting pay analysis. Table 7, Column 3 indicates a statistically significant female shortfall in starting pay of 3.32% when controlling for detailed worker characteristics and Job Subfamily. When controls for Job Level are added (Column 4), a statistically significant shortfall remains but it declines to 1.18%. The difference between these two statistically significant shortfalls indicates Nike is hiring women into lower Job Levels.

104. The hiring of women at lower-paying Job Levels remains consequential in the class period. The same comparison with respect to the analysis of class period pay – between the disparity controlling for Job Level and the disparity without these controls (see Table 2 and 8) – indicates that this initial disadvantage for women persists into the class period.

105. Finally, I show this by looking directly at the Job Levels into which people are hired. I begin with descriptive information in Figure 1 (p. 65), which shows clearly that women are hired into lower Job Levels throughout the Job Level structure.

    a. The two curves in the figure – one for women (orange) and one for men (blue) – show the cumulative percentage of either all women or all men hired at a given Job Level or the lower ones to the left. Note that both curves start at 0% and end at 100%, reflecting the fact that all employees have to be hired at some Job Level. And

as we include successively higher Job Levels, an increasing share have to be hired at or below that level – reaching 100% at the highest Level.

b. For example, consider the values at "Supervisor." As one can read off the figure, or see in the data below the figure, 40.36% of women are hired at this Job Level or the Job Levels below, vs. only 26.80% of men. Hence the line for women is *higher* for women, which implies that considering the Job Levels up to and including this one, women are hired at *lower* Job Levels. The curve for women is above the curve for men for until we get to the very highest Job Levels. This is reflected, in the data below the figure, in the higher "Female, %" than "Male, %" entries, showing that women are over-represented in the lowest Job Levels.[81]

106. Next, I estimate regression models for the beginning Job Level. Job Level is what we call an "ordinal" variable, rather than a "cardinal" variable. An ordinal variable has a clear ranking – like Job Levels – but not a numerical measurement (like temperature or money). Although there are more complex methods to estimate models for ordinal data, they are difficult to interpret. I thus estimate models treating Job Level as cardinal variable, from 1 to 11 (with Senior Director, the highest Job Level in the class, coded as 11).

a. It is worth reiterating what I said in my discussion of the analysis of starting pay. Just like I do not have starting pay information before January 1, 2012, I also do not have starting Job Level information, because of – as I understand it – limitations on what Nike provided. All else the same, the smaller number of observations with starting pay would reduce the standard deviations and statistical significance of any gender

[81] As explained above, the curves have to meet at the highest Job Level. The fact that they do not meet until very high Job Levels (Expert Professional) is a reflection of the over-representation of women at lower Job Levels.

disparity I estimate.

107. The estimates in column 1 of Table 12 are for the regression model including individual controls and Job Subfamily, paralleling what I did for starting pay (although of course I now never include Job Level as a control, since that is the outcome). The estimate indicates that women are hired at significantly lower Job Levels (2.16 SDs). The estimate of −0.1087 implies that women are hired, on average, at a Job Level just over one-tenth of a level lower than men.

108. In column 2 of Table 12, I add the detailed prior experience and education controls. The estimated difference in starting job level grows, nearly doubling, and is more strongly statistically significant (2.94 SDs).

109. In columns 3 and 4 of Table 12, I split the sample into those hired before September 2017, and those hired after. This corresponds to when Nike states it stopped asking about prior pay when hiring (see footnote 56), which could have influenced the Job Levels at which people were hired. (When prior pay information was collected, it could have resulted in women being hired into lower Job Levels based on lower prior pay.) There is a clear difference, with women hired at lower Job Levels in the early period (the estimate is now nearly one-third of a Job Level lower, and 3.39 SDs). But for those hired later, there is no statistically significant difference.

110. Like for starting pay, my preferred analysis of starting Job Level is the analysis in columns 2-4, which uses the information on schools and prior job titles in the most rigorous way that better characterizes the education and prior job histories of new hires. The results of this analysis show that women are hired into lower Job Levels than otherwise similar men, and the difference is statistically significant. This is exactly the phenomenon that helps explain

why my models for compensation that do not control for Job Level yield considerably larger pay shortfalls for women.

111. This evidence on Nike channeling women into lower paying jobs is consistent with statements and admissions by Nike executives, spokespeople, and publications that have acknowledged bias in Nike's hiring, the need to improve HR processes that have "underserved" Nike employees, Nike's broader need to remove barriers to access to jobs and to increase the representation of women "at all levels."[82]

[82] Ex. 512, email from Monique Matheson, April 4, 2018, NIKE_00002233. In April 2018, Nike's CHRO also wrote that "we need to improve representation of women," "[w]hile we've spoken about this many times, and tried different ways to achieve change, we have failed to gain traction – and our hiring and promotion decisions are not changing senior-level representation as quickly as we have wanted," and confirmed that Nike was going to "remove bias from critical moments of the hiring process by creating more inclusive job descriptions …" (Ex. 633 at NIKE_00003198-99.) Nike's CHRO testified that its inclusive jobs descriptions "are intended to remove the types of things that can create unnecessary or artificial barriers to access to the jobs." (Matheson Dep. 197:8-22.) Nike made the same statements to reporters (e.g., NIKE_00019431) and in its FY 16/17 Sustainable Business Report (Ex. 509 at NIKE_00002052). Nike's CEO, Mark Parker, acknowledged Nike's HR failings and the promised "to improve processes that underserved us in recent years … and to restore trust in places where it has eroded." (Ex. 561 at NIKE_00001966.)

Table 1: Source Data Files and Analysis Data Sets

Source data files	*Description*	*Observations*	*Period*
Snapshot data	Monthly snapshot of employees	726,359 person-months	July 2012-September 2019
Static file	Unique employee records and status	13,397 employees	As of August 31, 2019 (including terminated employees)
Comp change file	Unique records for all bonuses and merit increases awarded to each employee	227,734 employee-awards	2012-May 2019
PSP file(s)	Unique records for each 2019 PSP bonus awarded to each employee	10,400 employee-PSP bonuses	2019
Merit Increase file	Unique records for each 2019 Merit increase awarded to each employee	9,927 employee-Merit increases	2019
Potential Appraisal Ratings	Unique records for potential appraisal ratings for each employee	5,749 employees	FY 2013-FY 2018
Talent Segmentation Ratings	Unique records for talent segmentation ratings for each employee	4,093 employees	FY 2018-FY 2019
Hire data	Unique records for each hire	14,486 hires	2012-2020
Application data	Record for each application make for employment at Nike	998,338 applications	2012-2020

Table 2: Estimated Gender Differences in Pay During Class Period (August 9, 2015-September 1, 2019)

	(1)	(2)	(3)	(4)
	Individual Controls	(1): Plus Job Subfamily	(2): Plus Job Levels	(1): Plus Interactions of Job Subfamily and Job Levels
A. Log of Base Pay				
Female shortfall	**-10.86%**	**-6.37%**	**-1.76%**	**-1.78%**
Implied $ shortfall	-$14,464	-$8,484	-$2,344	-$2,371
Std. deviations	17.80	12.06	8.35	8.97
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	43,555	43,550	43,550	43,484
B. Log of PSP Bonuses				
Female shortfall	**-19.41%**	**-12.45%**	**-3.14%**	**-3.19%**
Implied $ shortfall	-$3,923	-$2,516	-$635	-$645
Std. deviations	15.49	11.18	5.53	5.66
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 100,000	< 1 in 100,000
Observations	39,766	39,764	39,764	39,691
C. Log of Base Pay and PSP Bonuses				
Female shortfall	**-12.01%**	**-7.35%**	**-2.01%**	**-2.03%**
Implied $ shortfall	-$18,568	-$11,363	-$3,107	-$3,138
Std. deviations	17.30	12.25	9.00	9.55
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	39,766	39,764	39,764	39,691
Controls (Panels A-C)				
Year Fixed Effects	x	x	x	x
Tenure	x	x	x	x
Tenure Squared	x	x	x	x
Age	x	x	x	x
Age Squared	x	x	x	x
Time in Job Code	x	x	x	x
Time in Job Code Squared	x	x	x	x
Job Performance Rating (6 categories)	x	x	x	x
Full Time/Part Time Status	x	x	x	x
Job Structure (refers to Panel A; can be slight differences for other panels)				
Job Subfamily (181 categories)		x	x	
Job Level (11 categories)			x	
Interaction of Job Subfamily and Job Level (972 categories)				x
Number of unique job structure categories		175	185	900

The estimated female shortfalls are based on regressions for the log of pay, and hence are approximate percentage differences. The standard deviations for all regression estimates are computed clustering at the individual level. Base Pay and PSP Bonuses are all converted to December 2019 dollars. In Panels B and C, observations with missing PSP bonuses are excluded. Implied $ shortfalls for women are estimated based on the mean pay for men in the relevant sample. The data consist of annual snapshots and cover the time period from 2015 to 2019. For the time period from 2015 to 2018, the snapshots are as of December 1 of each year. For 2019, the snapshots are as of September 1. The class consists of salaried, corporate positions in Nike headquarters in Oregon that are or were lower-level positions than Vice-President excluding Nike retail store employees, lawyers within Nike's legal department, and employees in Nike's finance and HR departments. Person-year observations outside the class are not included. Regressions include a dummy variable for each performance rating category (including a dummy for missing performance rating). Job Performance Rating has six categories: Consistent Rating, Successful Rating, Highly Successful Rating, Exceptional Rating, Unsatisfactory Rating, and the combined category of Not Rated, Too New To Rate, and missing rating.

Data Sources: Snapshot Data, Static File, Comp Change File, PSP File.

Table 3: Estimated Gender Differences in Pay During Class Period (August 9, 2015-September 1, 2019)

	(1)	(2)	(3)	(4)	(5)
	Individual Controls	(1): Plus Tenure, Time in Job Code	(1): Plus Performance Ratings	(2): Plus Performance Ratings	(1): Plus Age
A. Log of Base Pay					
Female shortfall	**-12.43%**	**-13.42%**	**-12.84%**	**-13.68%**	**-10.34%**
Implied $ shortfall	-$16,555	-$17,873	-$17,101	-$18,219	-$13,771
Std. deviations	17.47	19.56	18.41	20.30	16.09
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	43,556	43,555	43,556	43,555	43,556
B. Log of PSP Bonuses					
Female shortfall	**-19.35%**	**-22.05%**	**-22.29%**	**-24.20%**	**-14.87%**
Implied $ shortfall	-$3,910	-$4,456	-$4,505	-$4,891	-$3,005
Std. deviations	12.63	15.27	15.7851	17.9061	10.7119
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	39,767	39,766	39,767	39,766	39,767
C. Log of Base Pay and PSP Bonuses					
Female shortfall	**-13.38%**	**-14.51%**	**-13.85%**	**-14.86%**	**-11.16%**
Implied $ shortfall	-$20,686	-$22,433	-$21,412	-$22,974	-$17,254
Std. deviations	16.61	18.73	17.54	19.57	15.21
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion
Observations	39,767	39,766	39,767	39,766	39,767
Controls (Panels A-C)					
Year Fixed Effects	x	x	x	x	x
Tenure		x		x	
Tenure Squared		x		x	
Age					x
Age Squared					x
Time in Job Code		x		x	
Time in Job Code Squared		x		x	
Job Performance Rating (6 categories)			x	x	
Full Time / Part Time Status	x	x	x	x	x

D. Averages

	Panel A		Panels B-C	
	Male	Female	Male	Female
Consistent Rating	[redacted]	[redacted]	[redacted]	[redacted]
Successful Rating	[redacted]	[redacted]	[redacted]	[redacted]
Highly Successful Rating	[redacted]	[redacted]	[redacted]	[redacted]
Exceptional Rating	[redacted]	[redacted]	[redacted]	[redacted]
Missing or Too New To Rate	[redacted]	[redacted]	[redacted]	[redacted]
Unsatisfactory Rating (Omitted Category)	[redacted]	[redacted]	[redacted]	[redacted]
Tenure	[redacted]	[redacted]	[redacted]	[redacted]
Time in Job Code	[redacted]	[redacted]	[redacted]	[redacted]
Age	[redacted]	[redacted]	[redacted]	[redacted]

The estimated female shortfalls are based on regressions for the log of pay, and hence are approximate percentage differences. The standard deviations for all regression estimates are computed clustering at the individual level. Base Pay and PSP Bonuses are all converted to December 2019 dollars. In Panels B and C, observations with missing PSP bonuses are excluded. Implied $ shortfalls for women are estimated based on the mean pay for men in the relevant sample. The data consist of annual snapshots and cover the time period from 2015 to 2019. For the time period from 2015 to 2018, the snapshots are as of December 1 of each year. For 2019, the snapshots are as of September 1. The class consists of salaried, corporate positions in Nike headquarters in Oregon that are or were lower-level positions than Vice-President excluding Nike retail store employees, lawyers within Nike's legal department, and employees in Nike's finance and HR departments. Person-year observations outside the class are not included. See notes to Table 2 for explanation of performance rating controls in column 3.

Data Sources: Snapshot Data, Static File, Comp Change File, PSP File.

Table 4: Estimated Gender Differences in Pay

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Period/Controls	2016-2019: Subfamily	2016-2019: Subfamily and Level	2016-2019: Interactions of Subfamily and Level	2017-2019: Subfamily	2017-2019: Subfamily and Level	2017-2019: Interactions of Subfamily Level	2015-2019: Hired before September 2017: Subfamily	September 2017-2019: Hired September 2017 or Later: Subfamily	2015-2019: Hired before September 2017: Interactions of Subfamily and Level	September 2017-2019: Hired in September 2017 or Later: Interactions of Subfamily and Level	2018-2019: Hired before September 2017: Subfamily	2018-2019: Hired before September 2017: Interaction of Subfamily and Level
A. Log of Base Pay												
Female shortfall	**-6.24%**	**-1.63%**	**-1.68%**	**-5.95%**	**-1.53%**	**-1.60%**	**-6.75%**	**-0.54%**	**-1.89%**	**-0.21%**	**-6.33%**	**-1.56%**
Implied $ shortfall	-$8,384	-$2,190	-$2,257	-$8,063	-$2,073	-$2,168	-$8,990	-$719	-$2,517	-$280	-$8,008	-$1,971
Std. deviations	11.68	7.67	8.38	10.87	7.03	7.74	11.98	0.46	8.88	0.47	9.79	6.53
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1.5	< 1 in 1 billion	< 1 in 1.5	< 1 in 1 billion	< 1 in 1 billion
Observations	36,195	36,195	36,122	27,827	27,827	27,741	39,880	3,654	39,811	3,566	15467	15369
B. Log of PSP Bonuses												
Female shortfall	**-12.21%**	**-2.90%**	**-2.97%**	**-11.72%**	**-2.98%**	**-3.35%**	**-13.01%**	**-1.70%**	**-3.26%**	**-0.79%**	**-13.20%**	**-4.09%**
Implied $ shortfall	-$2,247	-$534	-$546	-$2,200	-$559	-$629	-$2,629	-$344	-$659	-$160	-$2,668	-$827
Std. deviations	10.45	4.50	4.65	9.85	4.40	4.94	11.33	0.45	6.03	0.23	9.66	5.20
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100,000	< 1 in 100,000	< 1 in 1 billion	< 1 in 100,000	< 1 in 1 million	< 1 in 1 billion	< 1 in 1.5	< 1 in 100 million	< 1 in 1.2	< 1 in 1 billion	< 1 in 1 million
Observations	33,307	33,307	33,233	25,644	25,644	25,560	37,263	2,490	37,186	2,390	14,983	14,890
C. Log of Base Pay and PSP Bonuses												
Female shortfall	**-7.13%**	**-1.86%**	**-1.91%**	**-6.83%**	**-1.80%**	**-1.88%**	**-7.67%**	**0.00%**	**-2.10%**	**-0.30%**	**-7.20%**	**-1.80%**
Implied $ shortfall	-$10,955	-$2,858	-$2,935	-$10,596	-$2,793	-$2,917	-$11,858	$1	-$3,247	-$464	-$11,124	-$2,776
Std. deviations	11.84	8.27	8.92	11.04	7.78	8.50	12.17	0.00	9.40	0.59	9.98	7.16
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1.0003	< 1 in 1 billion	< 1 in 1.8	< 1 in 1 billion	< 1 in 1 billion
Observations	33,307	33,307	33,233	25,644	25,644	25,560	37,263	2,490	37,186	2,390	14,983	14,890

The estimated female shortfalls are based on regressions for the log of pay, and hence are approximate percentage differences. The standard deviations for all regression estimates are computed clustering at the individual level. Base Pay and PSP Bonuses are all converted to December 2019 dollars. All specifications include controls in Table 2, column 1, as well as controls indicated in column headings. In Panels B and C, observations with missing PSP bonuses are excluded. Implied $ shortfalls for women are estimated based on the mean pay for men in the relevant sample. The data consist of annual snapshots and cover the time period from 2016 to 2019. For the time period from 2016 to 2018, the snapshots are as of December 1 of each year. For 2019, the snapshots are as of September 1. The class consists of salaried, corporate positions in Nike headquarters in Oregon that are or were lower-level positions than Vice-President excluding Nike retail store employees, lawyers within Nike's legal department, and employees in Nike's finance and HR departments. Person-year observations outside the class are not included.

Data Sources: Snapshot Data, Static File, Comp Change File, and PSP file.

Table 5: Estimated Gender Differences in Pay During Class Period by Year

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Period/Controls	2015: Subfamily	2015: Interactions of Subfamily and Level	2016: Subfamily	2016: Interactions of Subfamily and Level	2017: Subfamily	2017: Interactions of Subfamily and Level	2018: Subfamily	2018: Interactions of Subfamily and Level	2019: Subfamily	2019: Interactions of Subfamily and Level
A. Log of Base Pay										
Female shortfall	**-7.01%**	**-2.11%**	**-7.11%**	**-1.87%**	**-6.70%**	**-2.02%**	**-5.86%**	**-1.64%**	**-5.24%**	**-1.12%**
Implied $ shortfall	-$8,923	-$2,686	-$9,275	-$2,439	-$8,887	-$2,679	-$7,911	-$2,214	-$7,265	-$1,553
Std. deviations	10.35	6.96	11.19	6.70	10.75	7.17	9.54	6.61	8.84	5.21
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 100 million	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 million
Observations	7,350	7,205	8,358	8,228	8,881	8,737	9,238	9,102	9,684	9,545
B. Log of PSP Bonuses										
Female shortfall	**-13.32%**	**-3.75%**	**-13.89%**	**-1.69%**	**-11.88%**	**-2.65%**	**-13.77%**	**-5.24%**	**-10.24%**	**-2.38%**
Implied $ shortfall	-$3,963	-$1,116	-$2,382	-$290	-$1,568	-$350	-$1,950	-$742	-$2,849	-$662
Std. deviations	8.43	3.51	7.99	1.31	8.29	2.93	7.95	3.63	7.22	2.49
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1,000	< 1 in 1 billion	< 1 in 5	< 1 in 1 billion	< 1 in 100	< 1 in 1 billion	< 1 in 1,000	< 1 in 1 billion	< 1 in 20
Observations	6,450	6,305	7,651	7,525	8,153	8,013	8,282	8,145	9,182	9,041
C. Log of Base Pay and PSP Bonuses										
Female shortfall	**-8.48%**	**-2.03%**	**-8.03%**	**-1.91%**	**-7.40%**	**-1.88%**	**-6.96%**	**-1.62%**	**-6.03%**	**-1.30%**
Implied $ shortfall	-$13,535	-$3,240	-$11,932	-$2,838	-$10,859	-$2,759	-$10,465	-$2,436	-$10,060	-$2,169
Std. deviations	10.30	9.55	11.15	8.92	10.76	8.50	9.96	7.16	8.81	5.60
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 100 million	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 1 billion	< 1 in 10 million
Observations	6,450	6,305	7,651	7,525	8,153	8,013	8,282	8,145	9,182	8,976

The estimated female shortfalls are based on regressions for the log of pay, and hence are approximate percentage differences. The standard deviations for all regression estimates are computed clustering at the individual level. Base Pay and PSP Bonuses are all converted to December 2019 dollars. All specifications include controls Table 2, column 1, as well as controls indicated in column headings. In Panels B and C, observations with missing PSP bonuses are excluded. Implied $ shortfalls for women are estimated based on the mean pay for men in the relevant sample. I use annual snapshots for the analysis by year. For the time period from 2015 to 2018, the snapshots are as of December 1 of each year. For 2019, the snapshots are as of September 1. The class consists of salaried, corporate positions in Nike headquarters in Oregon that are or were lower-level positions than Vice-President excluding Nike retail store employees, lawyers within Nike's legal department, and employees in Nike's finance and HR departments. Person-year observations outside the class are not included.
Data Sources: Snapshot Data, Static File, Comp Change File, and PSP file.

Table 6: Estimated Gender Differences in Starting Pay (January 1, 2012-September 1, 2019)

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual controls	(1): Plus Job Subfamily	(2): Plus Job Levels	(1): Plus Interactions of Job Subfamily and Job Levels	Same as (4): Before September 2017	Same as (4): September 2017 or Later
Log of Starting Pay						
Female shortfall	**-7.54%**	**-2.08%**	**-0.79%**	**-0.95%**	**-1.06%**	**-0.48%**
Implied $ shortfall	-$8,756	-$2,416	-$917	-$1,103	-$1,231	-$557
Std. deviations	9.86	3.00	2.34	2.90	2.39	1.00
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 100	< 1 in 20	< 1 in 100	< 1 in 20	< 1 in 3
Observations	6,712	6,688	6,687	6,457	4,382	1,922
Controls						
Year Fixed Effects	x	x	x	x	x	x
Age	x	x	x	x	x	x
Age Squared	x	x	x	x	x	x
Full Time / Part Time Status	x	x	x	x	x	x
Job Structure						
Job Subfamily (215 categories)		x	x			
Job Level (12 categories)			x			
Interaction of Job Subfamily and Job Level (824 categories)				x	x	x
Number of unique job structure controls		188	198	551	468	258

The estimated female shortfall is calculated by multiplying the average male starting base pay by the estimated female difference in log pay. The analysis includes starting pay for employees initially hired on or after January 1, 2012, who were class members at some point during the class period, including those whose starting position was in a department or location outside the class (e.g., in the Human Resources department or outside Nike headquarters in Oregon). The analysis excludes employees who started in the V and A bands as their starting pay was mostly hourly. Starting pay is converted to December 2019 dollars.

Data Sources: Snapshot data, Static file.

Table 7: Estimated Gender Differences in Starting Pay (January 1, 2012-September 1, 2019)

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual Controls Plus Job Subfamily	(1): Plus Interactions of Job Subfamily and Job Level	Individual Control, Job Subfamily, Experience by Clusters of Job Titles, Highest Education Level, & University Rankings	Individual Controls, Interaction of Job Subfamily and Job Level, Experience by Clusters of Job Titles, Highest Education Level, & University Rankings	Same as (4): Before September 2017	Same as (4): September 2017 or Later
Log of Starting Pay						
Female shortfall	**-2.22%**	**-1.09%**	**-3.32%**	**-1.18%**	**-1.33%**	**-0.56%**
Implied $ shortfall	-$2,573	-$1,262	-$3,852	-$1,368	-$1,540	-$649
Std. deviations	2.83	3.17	3.84	3.31	2.70	1.00
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 100	< 1 in 100	< 1 in 1,000	< 1 in 1,000	< 1 in 100	< 1 in 3
Observations	4,902	4,701	4,859	4,657	2,889	1,622
Controls						
Year Fixed Effects	x	x	x	x	x	x
Age	x	x				
Age Squared	x	x				
Full Time/Part Time Status	x	x	x	x	x	x
Job Structure						
Job Subfamily (215 categories)	x		x			
Interaction of Job Subfamily and Job Level (824 categories)		x		x	x	x
Number of unique job structure controls	161	466	160	464	365	244
Education Level			x	x	x	x
University Rankings*			x	x	x	x
Prior Work Experience by Clusters of Job Titles**			x	x	x	x
Prior Work Experience by Clusters of Job Titles Squared			x	x	x	x

The estimated female shortfall is calculated by multiplying the average male starting base pay by the estimated female difference in log pay. The analysis includes starting pay for employees initially hired on or after January 1, 2012, who were class members at some point during the class period including those whose starting position was in the department or location outside the class (e.g., in the Human Resources department or outside Nike headquarters in Oregon). The analysis excludes employees who started in the V and A bands as their starting pay was mostly hourly. Education and prior job experience is available only for a subset of observations matched with the job applications. 4,030 observations were matched with job applications that got them hired. The remaining 880 observations could not be matched with job applications that got them hired but they were matched with job applications submitted before the initial hire date. Starting pay is converted to December 2019 dollars.

*There are three university rankings included in the regressions: the WSJ Rankings, the QS World University Rankings, and the Center for World University Rankings (CWUR).

**There are 20 clusters of job titles.

Data Sources: Snapshot data, Static file, Hire Data, and Applications data.

Table 8: Estimated Gender Differences in Base Pay during the Class Period (August 9, 2015-September 01, 2019) for the Sample Matched with the Job Applications

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual Controls Plus Job Subfamily	Individual Controls, Job Subfamily, Experience by Clusters of Job Titles, Highest Education Level, & University Rankings	Individual Controls, Interactions of Job Subfamily and Job Level	Individual Controls, Interactions of Job Subfamily and Job Level, Experience by Clusters of Job Titles, Highest Education Level, & University Rankings	Same as (4): Hired before September 2017	Same as (4): Hired in September 2017 or Later
Log of Base Pay						
Female shortfall	**-3.51%**	**-4.52%**	**-1.10%**	**-1.03%**	**-1.07%**	**-0.59%**
Implied $ shortfall	-$4,348	-$5,600	-$1,363	-$1,278	-$1,322	-$725
Std. deviations	4.39	5.16	3.50	3.29	2.83	1.13
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 10,000	< 1 in 1 million	< 1 in 1,000	< 1 in 1,000	< 1 in 100	< 1 in 3
Observations	14,563	14,563	14,494	14,494	11,649	2,757
Controls						
Year Fixed Effects	x	x	x	x	x	x
Age	x		x			
Age Squared	x		x			
Tenure	x	x	x	x	x	x
Tenure Squared	x	x	x	x	x	x
Time in Job Code	x	x	x	x	x	x
Time in Job Code Squared	x	x	x	x	x	x
Job Performance Rating (6 categories)	x	x	x	x	x	x
Full Time / Part Time Status	x	x	x	x	x	x
Job Structure						
Job Subfamily (181 categories)	x	x				
Interaction of Job Subfamily and Job Level (972 categories)			x	x	x	x
Number of unique job structure controls	151	151	658	658	612	317
Education Level		x		x	x	x
University Rankings*		x		x	x	x
Prior Work Experience by Clusters of Job Titles**		x		x	x	x
Prior Work Experience by Clusters of Job Titles Squared		x		x	x	x

The estimated female shortfalls are based on regressions for the log of pay, and hence are approximate percentage differences. The standard deviations for all regression estimates are computed clustering at the individual level. Base Pay is converted to December 2019 dollars. Implied $ shortfalls for women are estimated based on mean pay for men in the relevant sample. The data consist of annual snapshots and cover the time period from 2016 to 2019. For the time period from 2016 to 2018, the snapshots are as of December 1 of each year. For 2019, the snapshots are as of September 1. The class consists of salaried, corporate positions in Nike headquarters in Oregon that are or were lower-level positions than Vice-President excluding Nike retail store employees, lawyers within Nike's legal department, and employees in Nike's finance and HR departments. I did not include person-year observations that are not in the class. The analysis includes employees initially hired on or after January 1, 2012, matched with the job applications.

*There are three university rankings included in the regressions: the WSJ Rankings, the QS World University Rankings, and the Center for World University Rankings (CWUR).

**There are 20 clusters of job titles.

Data Sources: Snapshot Data, Static File, Hire Data, and Applications Data.

Table 9: Estimated Gender Differences in Merit Pay Increase (January 1, 2013-September 1, 2019)

	(1)	(2)
	Individual Controls Plus Job Subfamily	Individual Controls Plus Interactions of Job Subfamily and Job Levels
A. Merit Amount Increase		
Female shortfall	**-$235.27**	**$5.11**
Std. deviations	8.85	0.37
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 1 billion	< 1 in 1.4
Observations	52,641	52,487
B. Log of Merit Amount Increase		
Female shortfall	**-2.96%**	**3.65%**
Std. deviations	1.62	3.51
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 9	< 1 in 1,000
Observations	52,641	52,487
Controls		
Year Fixed Effects	x	x
Tenure in t-1	x	x
Tenure Squared in t-1	x	x
Time in Job Code in t-1	x	x
Time in Job Code Squared in t-1	x	x
Age in t-1	x	x
Age Squared in t-1	x	x
Job Performance Rating (6 categories) in t-1	x	x
Full Time / Part Time Status in t-1	x	x
Job Structure (refers to Panel A; can be slight differences for other panels)		
Job Subfamily (238 categories) in t-1	x	
Interaction of Job Subfamily and Job Level (1516 categories) in t-1		x
Number of unique job structure categories	230	1280

The standard deviations for all regression estimates are computed clustering at the individual level. Merit Pay Increases are all converted to December 2019 dollars. The data consist of annual merit pay increases and cover the time period from 2012 to 2019. The analysis includes all employees who were class members at some point during the class period. Merit Amount Increase is a variable from the Comp Change File and Merit Pay Increase File. 2012 is excluded because it is the first year in the data and controls in t-1 cannot be determined for 2012. Observations in the Snapshot Data but not in the Merit file are not included in the regressions.

Data Sources: Snapshot Data, Static File, Comp Change File, and Merit Pay Increase File.

Table 10: Estimated Gender Differences in Promotions (January 1, 2013-September 1, 2019)

	(1)	(2)	(3)	(4)	(5)
	Individual Controls	Individual Controls Plus Job Subfamily	Individual Controls Plus Interactions of Job Subfamily and Job Level	Same as (3): Pre-April 2018	Same as (3): April 2018 and Later
A. Promotion Rate: All Promotions					
Female coefficient	**0.0039**	**0.0039**	**-0.0042**	**-0.0046**	**0.0003**
Implied % change	2.72%	2.72%	-2.89%	-3.18%	0.18%
Std. deviations	1.39	1.34	1.31	1.25	0.04
Observations	51,978	51,968	51,868	41,334	10,323
B. Promotion Rate: Only Competitive Promotions					
Female coefficient	**0.0080**	**0.0052**	**0.0030**	**0.0042**	**-0.0017**
Implied % change	23.88%	15.52%	8.96%	12.54%	-5.07%
Std. deviations	4.78	2.94	1.61	1.95	0.44
Observations	51,978	51,968	51,868	41,334	10,323
C. Promotion Rate: Only Non-competitive Promotions					
Female coefficient	**-0.0042**	**-0.0012**	**-0.0072**	**-0.0087**	**0.0020**
Implied % change	-3.81%	-1.09%	-6.54%	-7.90%	1.82%
Std. deviations	1.63	0.46	2.50	2.61	0.38
Observations	51,978	51,968	51,868	41,334	10,323
Controls (Panels A - C)					
Year Fixed Effects	x	x	x	x	x
Age	x	x	x	x	x
Age Squared	x	x	x	x	x
Tenure	x	x	x	x	x
Tenure Squared	x	x	x	x	x
Time in Position	x	x	x	x	x
Time in Position Squared	x	x	x	x	x
Full Time / Part Time Status	x	x	x	x	x
Job Performance Rating	x	x	x	x	x
Job Structure (refers to Panel A; can be slight differences for other panels)					
Job Subfamily (228 categories)		x			
Interaction of Job Subfamily and Job Level (1132 categories)			x	x	x
Number of unique job structure controls		227	1131	1057	743

A promotion is a job change to a higher job level. A competitive promotion is a job change to a higher job level that can be matched with job openings in the Hire Data. A non-competitive promotion is a job change to a higher level that cannot be matched with job openings in the Hire Data. The standard deviations for all regression estimates are computed clustering at the individual level. Implied % change for women are estimated based on the mean promotion rate in the relevant sample. The analysis excludes 2012 because many job codes got assigned to different job levels in April 2013. Some job codes got assigned to different job levels in 2014, 2017, and 2018 but those instances were not excluded. Band levels V and A are excluded from the analysis.

Data Sources: Snapshot Data, Static File, and Hire Data.

Table 11: Estimated Gender Differences in Promotions by More than One Level (January 1, 2013-September 1, 2019)

	(1)	(2)	(3)
	Two or More Levels: Individual Controls Plus Interactions of Job Subfamily and Job Level	Three or More Levels: Individual Controls Plus Interactions of Job Subfamily and Job Level	Four or More Individual Controls Plus Interactions of Job Subfamily and Job Level
A. Promotion Rate: All Promotions			
Female coefficient	**-0.0067**	**-0.0048**	**-0.0015**
Implied % change	-7.84%	-10.41%	-7.10%
Std. deviations	2.64	2.57	1.06
Observations	51,868	51,868	51,868
B. Promotion Rate: Only Competitive Promotions			
Female coefficient	**0.0003**	**-0.0007**	**0.0001**
Implied % change	-1.33%	-5.51%	1.67%
Std. deviations	0.23	0.58	0.10
Observations	51,868	51,868	51,868
C. Promotion Rate: Only Non-Competitive Promotions			
Female coefficient	**-0.0071**	**-0.0042**	**-0.0016**
Implied % change	-11.18%	-12.43%	-10.46%
Std. deviations	3.20	2.58	1.32
Observations	51,868	51,868	51,868
Controls			
Year Fixed Effects	x	x	x
Tenure	x	x	x
Tenure Squared	x	x	x
Age	x	x	x
Age Squared	x	x	x
Time in Position	x	x	x
Time in Position Squared	x	x	x
Full Time / Part Time Status	x	x	x
Job Performance Rating	x	x	x
Job Structure (refers to Panel A; can be slight differences for other panels)			
Interaction of Job Subfamily and Job Level (1132 categories)	x	x	x
Number of unique job structure controls	1131	1131	1131

A promotion is a job change to a higher job level. A competitive promotion is a job change to a higher job level that can be matched with job openings in the Hire Data. A non-competitive promotion is a job change to a higher level that cannot be matched with job openings in the Hire Data. The standard deviations for all regression estimates are computed clustering at the individual level. Implied % change for women are estimated based on the mean promotion rate in the relevant sample. The analysis excludes 2012 because many job codes got assigned to different job levels in April 2013. Some job codes got assigned to different job levels in 2014, 2017, and 2018 but those instances were not excluded. Band levels V and A are excluded from the analysis.

Data Sources: Snapshot Data, Static File, and Hire Data.

Table 12: Estimated Gender Differences in Starting Job Levels (January 1, 2012-September 1, 2019)

	(1)	(2)	(3)	(4)
	Full Sample: Individual Controls Plus Job Subfamily	Sample Matched with Job Applications: Individual Controls, Job Subfamily, Experience by Clusters of Job Titles, Highest Education Level, & University Rankings	Sames as (2): Hired before September 2017	Sames as (2): Hired in September 2017 or Later
Job Level				
Female coefficient	**-0.1087**	**-0.1861**	**-0.2680**	**-0.0579**
Std. deviations	2.16	2.94	3.39	0.53
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 20	< 1 in 100	< 1 in 1000	< 1 in 1.6
Observations	6,819	4,873	3,082	1,770
Year Fixed Effects	x	x	x	x
Age	x			
Age Squared	x			
Full Time / Part Time Status	x	x	x	x
Education Level		x	x	x
University Rankings*		x	x	x
Prior Work Experience by Clusters of Job Titles**		x	x	x
Prior Work Experience by Clusters of Job Titles Squared		x	x	x
Job Structure				
Job Subfamily (228 categories)	x	x	x	x
Number of unique job structure controls	190	160	135	111

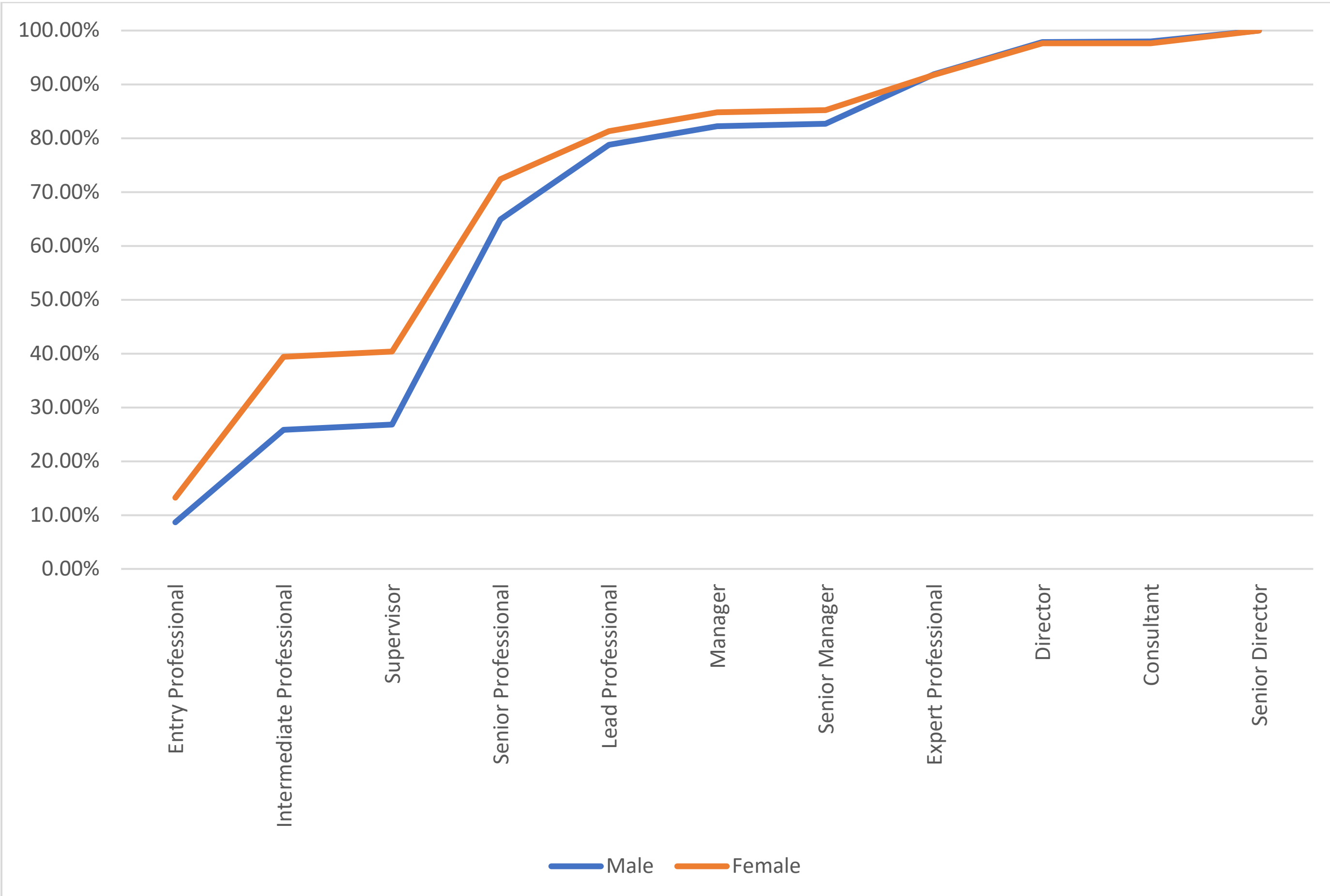
Dependent variable is job level coded as a numeric value from the lowest to the highest (e. g., Entry Professional is coded as 1 and Senior Director is coded as 11). The standard deviations for all regression estimates are computed clustering at the individual level. Band levels V and A are excluded from the analysis.

*There are three university rankings included in the regressions: the WSJ Rankings, the QS World University Rankings, and the Center for World University Rankings (CWUR).

**There are 20 clusters of job titles.

Data Sources: Snapshot Data, Static File, Hire Data, and Application Data.

Figure 1: Cumulative Distribution of New Hires by Job Level They Were Hired Into (2012-2019)



Job Level Hired Into	Male	Male, %	Female	Female, %	Cumulative Male %	Cumulative Female %
Entry Professional	354	8.66%	365	13.23%	8.66%	13.23%
Intermediate Professional	701	17.16%	722	26.18%	25.82%	39.41%
Supervisor	40	0.98%	26	0.94%	26.80%	40.36%
Senior Professional	1558	38.13%	883	32.02%	64.93%	72.37%
Lead Professional	567	13.88%	247	8.96%	78.81%	81.33%
Manager	141	3.45%	97	3.52%	82.26%	84.84%
Senior Manager	18	0.44%	10	0.36%	82.70%	85.21%
Expert Professional	376	9.20%	181	6.56%	91.90%	91.77%
Director	243	5.95%	161	5.84%	97.85%	97.61%
Consultant	5	0.12%	1	0.04%	97.97%	97.64%
Senior Director	83	2.03%	65	2.36%	100.00%	100.00%
Total	4086	100.00%	2758	100.00%		

The original Report was submitted on July 15, 2021. This corrected report is submitted on August 5, 2021.

David Neumark, Ph.D.

Appendix A: Materials

Nike Data

Comp_Change_thru_20190531_ Attorneys_Eyes_Only.txt

Comp_Change_thru_20190531_ Attorneys_Eyes_Only.xlsx

Comp_Change_2019_Equity_Attorneys_Eyes_Only.txt

Comp_Change_2019_Merit_Attorneys_Eyes_Only.txt

Comp_Change_2019_PSP_Attorneys_Eyes_Only.txt

Comp Change 2019 – Equity, Merit, PSP.xlsx

Pot_Appraisal_FY13_FY18_Attorneys_Eyes_Only.txt

Pot_Appraisal_FY13_FY18_Attorneys_Eyes_Only.xlsx

Segmentation_FY18_FY20_Attorneys_Eyes_Only.txt

Segmentation_FY18_FY20_Attorneys_Eyes_Only.xlsx

Snapshots_201207_201909_ Attorneys_Eyes_Only.txt

Snapshots_201207_201909_ Attorneys_Eyes_Only.xlsx

Static_Table_20190831_ Attorneys_Eyes_Only.txt

Static_Table_20190831_ Attorneys_Eyes_Only.xlsx

Taleo_Application_Data_2012_Attorneys_Eyes_Only.txt

Taleo_Application_Data_2013_Attorneys_Eyes_Only.txt

Taleo_Application_Data_2014_Attorneys_Eyes_Only.txt

Taleo_Application_Data_2015_Attorneys_Eyes_Only.txt

Taleo_Application_Data_2016_Attorneys_Eyes_Only.txt

Taleo_Application_Data_2017_Attorneys_Eyes_Only.txt

Taleo_Application_Data_2018_Attorneys_Eyes_Only.txt

Taleo_Application_Data_2019_Attorneys_Eyes_Only.txt

Taleo_Application_Data_2020_Attorneys_Eyes_Only.txt

Taleo_Application_Data_2012-2020_Attorneys_Eyes_Only.xlsx

Taleo_Hires_Data_Attorneys_Eyes_Only txt

Taleo_Hires_Data_Attorneys_Eyes_Only xlsx

Documents

First Amended Class and Collective Action Allegation Complaint, filed 11/19/18

JobCodeHistory.xlsx

Paul Hastings December 30, 2019 Letter re: *Cahill et al. v. Nike, Inc.* – Production of Nike Data Files

Paul Hastings November 1, 2020 Letter re: *Cahill, et al. v. Nike, Inc.* – Production of Taleo Data Files

Paul Hastings November 12, 2020 Letter re: *Cahill et al. v. Nike, Inc.*, Case No. 3:18-cv-01477-JR – Your questions regarding Nike's Taleo data production

Paul Hastings November 23, 2020 Letter re: *Cahill et al. v. Nike, Inc.*, Case No. 3:18-cv-01477-JR – Production of Data Files with Employee Names and ID Numbers

Deposition of Treasure Heinle

Deposition of Shine Thomas

Deposition of Shane Walker

Deposition of Monique Matheson

Deposition of Jessica Stuckey

Deposition of Shelli White

Deposition of Elizabeth Vales

Exhibit 500, "Total Rewards Fundamentals: Managing Pay at Nike," NIKE _00003191

Exhibit 504, "Bands and Levels," NIKE_00002393

Exhibit 507, "Nike Job Architecture," NIKE_0023548

Exhibit 509, "Maximum Performance Minimum Impact," NIKE_00001996

Exhibit 512, email from Monique Matheson, April 4, 2018, NIKE_00002233

Exhibit 513, "TA Comp History Policy," August 24, 2017, NIKE_00002070

Exhibit 515, "FY20 Total Rewards Update," NIKE_00024195

Exhibit 522, "Annual Pay Review Deep Dive," NIKE_00024770

Exhibit 523, "Approximate Timing of Compensation Processes & Program Changes," NIKE_00033376

Exhibit 530, "Annual Pay Review," NIKE_C_00003279

Exhibit 534, "Annual Pay Review Key Terms," NIKE _ 00003273

Exhibit 550, "Job Changes," NIKE_00002321

Exhibit 552, email from Monique Matheson, July 23, 2018, NIKE_00001647

Exhibit 559, "Year End Process: Annual Pay Review," NIKE_00003314

Exhibit 561, Version of Mark Parker's May 2018 Speech, NIKE_00001960

Exhibit 591, "Total Rewards Deep-Dive," NIKE_00001653

Exhibit 603, "Performance Rewards: CFE Collection FAQs for Manager," NIKE_00026617

Exhibit 614, "Performance Rewards FY16," NIKE_00030252

Exhibit 633, "Representation and Pay Equity Commitments," NIKE_00003198

Exhibit 638, "Assessing Performance Summary," NIKE_00002301

Exhibit 643, "Workforce Planning," NIKE_C_00001853

Exhibit 644, "Facilitation Materials," NIKE_00015839

Exhibit 645, "Talent Segmentation One-Pager," NIKE _ 00013810

Exhibit 647, "Nike Talent Planning," NIKE_00027184

Exhibit 648, "Workforce Planning FAQs – HR," NIKE_00001849

Exhibit 650, "Performance Potential Education Deck," NIKE_00015874

Exhibit 673, email from Daniel Laboe, October 12, 2017, NIKE_00024727

Exhibit 682, "Talk With Me: Bands & Levels," PLF_001989-90

Exhibit 711, "Brand Creative Org Evolution," NIKE_00038707

"FY16 PSP 2-Factor Payout Examples," April 7, 2016, NIKE_00030282

"Talk With *me*: Stock Options," April 21, 2016, NIKE_00030274

Email from Ilana Finley, April 5, 2018, NIKE_00019431

Labor Economics Research

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Other Data

Data on CPI-U from https://www.bls.gov/cpi/data.htm.

Appendix B: Supplemental Tables/Figures

Table B1: Regressions for Performance Ratings During Class Period (August 9, 2015-September 1, 2019)

	(1)	(2)	(3)	(4)	(5)
	Ordered ratings	Dummy for Exceptional Rating	Dummy for Highly Successful Rating or Higher	Dummy for Successful Rating or Higher	Dummy for Inconsistent Rating or Higher
Female shortfall	**0.0506**	**0.0017**	**0.0386**	**0.0094**	**0.0009**
Std. deviations	6.92	1.04	6.22	5.69	2.55
Probability of observing this estimate under null hypothesis of no gender difference	< 1 in 1 billion	< 1 in 3	< 1 in 1 billion	< 1 in 10 million	< 1 in 20
Observations	39,154	39,154	39,154	39,154	39,154
Controls					
Year Fixed Effects	x	x	x	x	x
Tenure	x	x	x	x	x
Tenure Squared	x	x	x	x	x
Time in Position	x	x	x	x	x
Time in Position Squared	x	x	x	x	x
Age	x	x	x	x	x
Age Squared	x	x	x	x	x
Full Time / Part Time Status	x	x	x	x	x
Job Structure					
Interaction of Job Subfamily and Job Level (972 categories)	x	x	x	x	x
Number of unique job structure categories	876	876	876	876	876

The data consist of annual snapshots and cover the time period from 2015 to 2019. For the time period from 2015 to 2018, the snapshots are as of December 1 of each year. For 2019, the snapshots are as of September 1. The class consists of salaried, corporate positions in Nike headquarters in Oregon that are or were lower-level positions than Vice-President excluding Nike retail store employees, lawyers within Nike's legal department, and employees in Nike's finance and HR departments. I did not include person-year observations when they are outside the class. Ordered ratings in column 1 are as follows: 5 Exceptional, 4 Highly Successful, 3 Successful, 2 Inconsistent, 1 Unsatisfactory. Not Rated, Too New To Rate, or missing ratings are excluded from the analysis.

Data Sources: Snapshot Data, Static File

Table B2: Regressions for Talent Segmentation Performance Ratings, Risk of Loss, Impact of Loss and Potential Appraisal Ratings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Ordered Ratings for Talent Segmentation (FY 2018 & FY 2019)	Talent Segmentation: Dummy for Accelerating Rating (FY 2018 & FY 2019)	Talent Segmentation: Dummy for Advancing Rating or Higher (FY 2018 & FY 2019)	Talent Segmentation: Dummy for Expanding Rating or Higher (FY 2018 & FY 2019)	Risk of Loss (FY 2018 & FY 2019)	Impact of Loss (FY 2018 & FY 2019)	Leadership Potential Assessment: Dummy for Hi Perf - Hi Po (FY 2016 through FY 2018)	Leadership Potential Assessment: Dummy for Hi Perf - Hi Po, Hi Perf - Mid Po or Mid Perf - Hi Po (FY 2016 through FY 2018)
Female shortfall	**0.1134**	**0.0177**	**0.0708**	**0.0248**	**-0.0059**	**-0.0246**	**-0.0070**	**0.0126**
Std. deviations	4.72	1.57	4.55	3.84	0.66	1.78	0.83	0.99
Probability of observing this estimate under null hypothesis of no gender difference	< 1 in 100,000	< 1 in 8	< 1 in 100,000	< 1 in 1,000	< 1 in 1.9	< 1 in 10	< 1 in 2.4	< 1 in 3
Observations	5,694	5,694	5,694	5,694	5,741	5,741	12,409	12,409
Controls								
Year Fixed Effects	x	x	x	x	x	x	x	x
Tenure	x	x	x	x	x	x	x	x
Tenure Squared	x	x	x	x	x	x	x	x
Time in Position	x	x	x	x	x	x	x	x
Time in Position Squared	x	x	x	x	x	x	x	x
Age	x	x	x	x	x	x	x	x
Age Squared	x	x	x	x	x	x	x	x
Full Time / Part Time Status	x	x	x	x	x	x	x	x
Job Structure								
Interaction of Job Subfamily and Job Level (972 categories)	x	x	x	x	x	x	x	x
Number of unique job structure categories	375	375	375	375	378	378	550	550

The data consist of annual snapshots and cover the time period from 2015 to 2019. For the time period from 2015 to 2018, the snapshots are as of December 1 of each year. For 2019, the snapshots are as of September 1. The class consists of salaried, corporate positions in Nike headquarters in Oregon that are or were lower-level positions than Vice-President excluding Nike retail store employees, lawyers within Nike's legal department, and employees in Nike's finance and HR departments. I did not include person-year observations when they are outside the class. Ordered ratings in column 1 are as follows: 4 Accelerating, 3 Advancing, 2 Expanding, 1 Transitioning. Too New To Rate or missing ratings are excluded from the analysis. Controls are as of December 1 of the previous year (e. g., for FY 2018 controls are as of December 1, 2017).

Data Sources: Snapshot Data, Static File, Talent Segmentation Ratings File, Potential Appraisal Ratings File.

Table B3: Descriptive Statistics for Matched and Unmatched Starting Pay Observations

	Starting Pay Sample Matched with the Applications	Starting Pay Sample Not Matched with the Applications
Observations	4,938	1,775
Average Starting pay	[redacted]	[redacted]
Female	[redacted]	[redacted]
Hire Year		
2012	2.47%	18.21%
2013	9.76%	8.74%
2014	11.36%	10.99%
2015	16.14%	21.87%
2016	15.03%	15.05%
2017	12.19%	10.20%
2018	19.34%	10.60%
2019	13.71%	4.34%
Bands		
L	31.71%	33.31%
U	51.82%	49.15%
E	14.46%	13.92%
S	2.00%	3.61%
Job Levels		
Entry Professional	9.62%	13.13%
Intermediate Professional	21.22%	18.88%
Supervisor	0.85%	1.30%
Senior Professional	36.07%	34.95%
Lead Professional	12.19%	11.22%
Manager	3.62%	2.93%
Expert Professional	8.10%	8.40%
Director	6.08%	5.36%
Consultant	0.04%	0.23%
Senior Manager	0.26%	0.73%
Senior Director	1.94%	2.87%
Job Functions		
Communications	1.76%	1.41%
Corporate & Government Affairs	0.55%	0.51%
Corporate Services	1.09%	1.07%
Data Analytics	0.97%	0.17%
Design	7.05%	13.13%
Digital	9.38%	5.52%
Finance	0.49%	4.62%
General Management	0.04%	0.11%
Human Resources	0.14%	0.79%
Information Technology	4.15%	5.58%
Legal	0.51%	0.73%
Logistics & Services	5.51%	6.31%
Manufacturing & Sourcing	5.49%	3.21%
Marketing	10.17%	8.40%
Merchandising	2.29%	1.63%
Product Creation	9.78%	9.02%
Product Management	0.83%	1.35%
Program/Process Excellence	7.15%	4.57%
Retail	3.73%	3.72%
Sales	3.36%	3.04%
Sports Management	0.38%	0.28%
Strategic Planning	2.05%	2.54%
Technology	23.15%	22.27%

Table B4: Descriptive Statistics for Matched Starting Pay Observations (Table 7)

	Male	Female
Observations	2,945	1,964
Highest Degree		
High School Diploma / GED	3.36%	1.02%
Other	3.53%	1.43%
Technical Diploma	0.81%	0.10%
Associates Degree	2.99%	2.04%
Non-degree Program	2.55%	1.43%
Bachelor's Degree	55.42%	65.63%
Master's Degree	28.59%	26.73%
Doctorate Degree	2.75%	1.63%
Years of prior experience	8.88	7.95

Table B5: Estimated Gender Differences in Starting Pay (January 1, 2012-September 1, 2019)

	(1)	(2)	(3)	(4)
	Individual Controls, Job Subfamily, Highest Education Level, Indicators for Most Common Universities, & Prior Experience by Job Title	Individual Controls, Interactions of Job Subfamily and Job Level, Highest Education Level, Indicators for Most Common Universities, & Prior Experience by Job Title	Same as (2): Before September 2017	Same as (2): September 2017 or Later
Log of Starting Pay				
Female shortfall	**-1.99%**	**-1.38%**	**-1.32%**	**-0.67%**
Implied $ shortfall	-$2,307	-$1,600	-$1,534	-$783
Std. deviations	2.21	4.05	2.42	0.87
Probability of observing this estimate under null hypothesis of no discrimination	< 1 in 20	< 1 in 10,000	< 1 in 20	< 1 in 2
Observations	4,859	4,657	2,886	1,613
Controls				
Year Fixed Effects	x	x	x	x
Full Time / Part Time Status	x	x	x	x
Job Structure				
Job Subfamily (215 categories)	x			
Interaction of Job Subfamily and Job Level (824 categories)		x	x	x
Number of unique job structure controls	160	464	365	242
Education Level	x	x	x	x
Indicators for each of the 118 most common schools**	x	x	x	x
Prior Work Experience by Job Title (252)***	x	x	x	x
Prior Work Experience by Job Title Squared	x	x	x	x

The estimated female shortfall is calculated by multiplying the average male base pay by the estimated female difference in log pay. The analysis includes starting pay for employees initially hired on or after January 1, 2012, who were class members at some point during the class period including those whose starting position was in the department or location outside the class (e.g., in the Human Resources department or outside Nike headquarters in Oregon). The analysis excludes employees who started in the V and A bands as their starting pay was mostly hourly. Education and prior job experience is available only for a subset of observations matched with the job applications. 4,030 observations were matched with job applications that got them hired. The remaining 881 observations could not be matched with job applications that got them hired; but they were matched with job applications submitted before the initial hire date. Starting pay is converted to December 2019 dollars.

**This captures 54% of all observations, and defines separate dummy variables for each school with 10 or more observations; the remainder are combined.

***This captures 30% of all observations, and defines separate variables for each job title with 10 or more observations; the remainder are combined.

Data Sources: Snapshot data, Static file, and Application data.

Table B6: Promotion Rates Out of Job Levels and Gender Composition of Job Levels (January 1, 2013-September 1, 2019)

Job Level	Male Promotion Rate Out of Each Job Level	Number of Males by Job Level	Distribution of Males by Job Level	Female Promotion Rate Out of Each Job Level	Number of Females by Job Level	Distribution of Females by Job Level	Total Number of Promotions
A: All Promotions							
Entry Professional	32.39%	1,584	5.4%	28.10%	1,801	8.0%	1,019
Intermediate Professional	21.33%	3,212	11.0%	18.15%	3,940	17.4%	1,400
Supervisor	7.11%	464	1.6%	10.20%	304	1.3%	64
Senior Professional	14.74%	8,644	29.7%	15.11%	7,050	31.1%	2,339
Lead Professional	15.32%	2,925	10.0%	15.81%	1,916	8.5%	751
Manager	14.65%	1,584	5.4%	11.42%	1,418	6.3%	394
Senior Manager	100.00%	2	0.01%	0.00%	0	0.0%	2
Expert Professional	15.45%	4,243	14.6%	18.41%	2,417	10.7%	1,092
Director	6.99%	4,335	14.9%	6.77%	2,644	11.7%	482
Consultant	20.00%	85	0.3%	19.05%	42	0.2%	25
Senior Director	2.23%	2,059	7.1%	4.35%	1,103	4.9%	94
Total	14.35%	29,135	100.00%	15.24%	22,635	100.0%	7,662
B: Only Competitive Promotions							
Entry Professional	9.41%	1,584	5.4%	9.22%	1,801	8.0%	315
Intermediate Professional	7.25%	3,212	11.0%	6.55%	3,940	17.4%	491
Supervisor	2.59%	464	1.6%	4.28%	304	1.3%	25
Senior Professional	3.32%	8,644	29.7%	4.38%	7,050	31.1%	596
Lead Professional	4.10%	2,925	10.0%	4.70%	1,916	8.5%	210
Manager	3.72%	1,584	5.4%	4.09%	1,418	6.3%	117
Senior Manager	0.00%	2	0.01%	0.00%	0	0.0%	0
Expert Professional	2.12%	4,243	14.6%	3.39%	2,417	10.7%	172
Director	0.67%	4,335	14.9%	0.72%	2,644	11.7%	48
Consultant	0.00%	85	0.3%	0.00%	42	0.2%	0
Senior Director	0.00%	2,059	7.1%	0.00%	1,103	4.9%	0
Total	3.34%	29,135	100.00%	4.36%	22,635	100.0%	1,974
C: Only Non-competitive Promotions							
Entry Professional	22.98%	1,584	5.4%	18.88%	1,801	8.0%	704
Intermediate Professional	14.07%	3,212	11.0%	11.60%	3,940	17.4%	909
Supervisor	4.53%	464	1.6%	5.92%	304	1.3%	39
Senior Professional	11.42%	8,644	29.7%	10.72%	7,050	31.1%	1,743
Lead Professional	11.21%	2,925	10.0%	11.12%	1,916	8.5%	541
Manager	10.92%	1,584	5.4%	7.33%	1,418	6.3%	277
Senior Manager	100.00%	2	0.01%	0.00%	0	0.0%	2
Expert Professional	13.13%	4,243	14.6%	15.02%	2,417	10.7%	920
Director	6.32%	4,335	14.9%	6.05%	2,644	11.7%	434
Consultant	20.00%	85	0.3%	19.05%	42	0.2%	25
Senior Director	2.23%	2,059	7.1%	4.35%	1,103	4.9%	94
Total	11.02%	29,135	100.00%	10.88%	22,635	100.0%	5,688

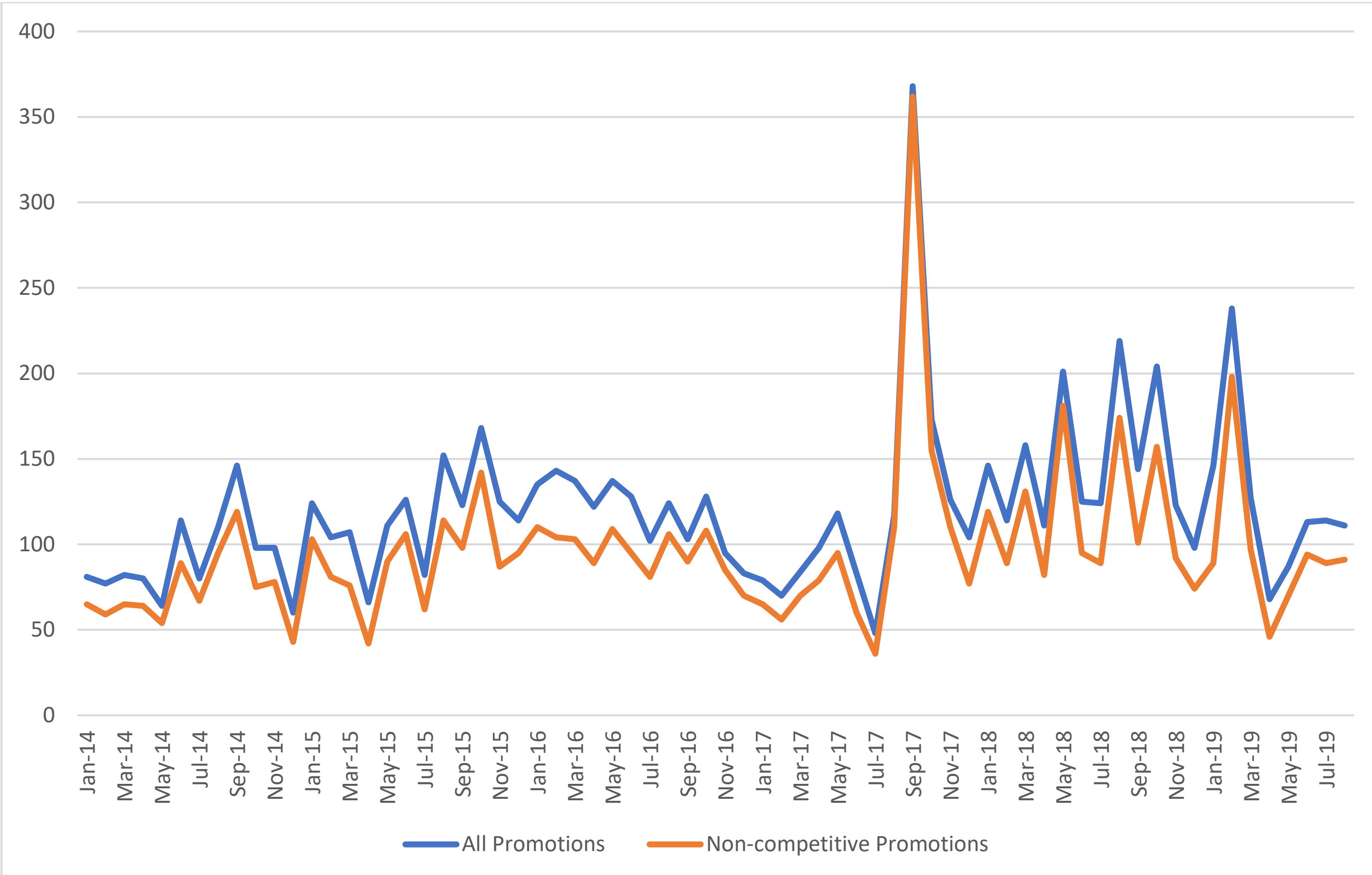
A promotion is a job change to a higher job level. A competitive promotion is a job change to a higher job level that can be matched with job openings in the Hire Data. A non-competitive promotion is a job change to a higher level that cannot be matched with job openings in the Hire Data. The analysis excludes 2012 because many job codes got assigned to different job levels in April 2013. Some job codes got assigned to different job levels in 2014, 2017, and 2018 but those instances were not excluded from the analysis. Band levels V and A are excluded from the analysis.

Data Sources: Snapshot Data, Static File, and Hire Data.

Table B7. Promotion Transition Matrix

	Level Promoted to										
Level Promoted from	ENTRY PROF.	INTER. PROF.	SUPER-VISOR	SENIOR PROF.	LEAD PROF.	MANAGER	EXPERT PROF.	DIRECTOR	CONSULTANT	SENIOR DIRECTOR	VICE PRESIDENT
ENTRY PROF.		600	19	330	47	19	2	2			
INTER. PROF.			30	1197	98	68	7				
SUPERVISOR				23	6	35					
SENIOR PROF.					1063	438	579	259			
LEAD PROF.						260	315	176			
MANAGER							84	309		1	
EXPERT PROF.								882	17	193	
DIRECTOR									11	468	3
CONSULTANT										24	1
SENIOR DIRECTOR											94

Figure B1: Time Series of Non-Competitive Promotions



Appendix C: School Names and Rankings

112. One of the additional educational attainment variables I controlled for in my starting pay and starting level analyses was the college or university where a job applicant obtained their highest degree (if any). In some models (Table B5), I include dummy variables for the most common schools in the data. However, this inevitably leaves many schools grouped together in a "catch-all" category. Thus, my preferred approach is to try to use the variation across all or most schools, which I do by assigning measurements of the quality of a given school based on well-established ratings and ranking systems.[83]

113. To attach schools in the data to available global rankings of colleges and universities, I first had to standardize the names of universities and colleges in the job application data. The data fields for prior schooling on Nike job applications, like the fields for prior job titles (discussed in Appendix D), are open-ended text fields. Thus, the same university may be represented in several different ways. I had to account for these variations and standardize the naming of schools to arrive at a more concise and accurate list of colleges and universities in the data to merge with ratings and raking systems.

114. This process required extensive data cleaning. Some job applicants only listed graduating from high school or obtaining a GED equivalent. I coded these observations as "High School." Instances where a job applicant listed "Other" for school, or a trade school equivalent (e.g., Culinary School), I coded as "Other." The methods I describe below were used for the remaining job applicants, who listed a college or university as the place where they obtained a given education qualification (Bachelor's, Master's, etc.). These methods

[83] There is academic research tying college quality to earnings, using rankings of colleges. See, for example: Eide, Eric, Michael J. Hilmer, and Mark H. Showalter. 2016. "Is It Where You Go or What You Study? The Relative Influence of College Selectivity and College Major on Earnings. *Contemporary Economic Policy*, Vol. 34, No. 1, pp. 37-46.

require some assumptions. I have used what seems best and most reasonable in determining the college or university an applicant listed on their job application.

115. Some job applicants used abbreviations or shorthand to reference universities and colleges on the applications, rather than the full name. For example, "UW" is the abbreviation for "the University of Washington."[84] Both names are used within the application data to reference the same university. Whenever there is a common shorthand name for a university within the job application data, I replace it with the full name of the university.

116. Some job applications not only give the name of the university they graduated from, but also the department within the university from where they obtained their degree. For example, an applicant may list they graduated from "Harvard University," while other applications cite graduating from the "Harvard Graduate School of Business" or "Harvard Law School" or "The Kennedy School of Government."[85] Whenever I find an applicant who listed a specific department within a university, I use the name of the university instead.

117. Wherever possible, I attempted to retain the specific campus a job applicant listed if it was part of a larger university system with several prominent universities. For example, there are many campuses within the "University of California" system.[86] Wherever possible, I attempted to identify if a job applicant obtained a degree from, say, "the University of California, Berkeley," as opposed to "the University of California, Irvine" or "the University of California, Los Angeles." The names of these specific campuses I also standardized, by removing abbreviations and the names of departments at a given campus. If a job applicant did not list a specific campus within a university system (say, they only listed "the

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[84] See https://www.washington.edu/.

[85] A full list of Harvard's programs can be found at: https://www.harvard.edu/academics/schools/.

[86] See https://www.universityofcalifornia.edu/uc-system/parts-of-uc.

University of California"), I assumed the applicant is referencing the flagship university in the school system.

118. Finally, I corrected for misspellings and errors in university names. As the name of the school was from an open-ended text field, the presence of such errors is unsurprising. In general, the bulk of these misspellings and errors were from missing or transposed letters in a university name. I also had to correct for errors I found with special characters, such as accent marks. These errors with special characters were most common in the names of international (i.e., non-U.S.) universities, such as the "University of São Paulo."

119. When I use dummy variables to simply construct indicators for schools, I created and included dummy variables for the most frequent schools, which I defined as any college or university with 10 or more observations in the data. These dummies covered 54% of observations.

120. Once I had a clear and uniform list of schools in the application data, I could match these schools to university ranking lists. For this analysis, I relied on three different data sets: the *Times Higher Education/Wall Street Journal* World University Rankings, the *QS* World University Rankings, and the *Center for World University Rankings*.[87] I extracted information on their rankings of universities for 2020. Each rating system is of a different length and applies different weighted metrics to generate an ordered ranking of universities. The *Times Higher Education/Wall Street Journal* World University Rankings contains 1,397 universities. The *QS* World University Rankings contains 1,024 universities. The *Center for World University Rankings* contains 2,000 universities. The universities are ranked in

[87] See https://www.timeshighereducation.com/world-university-rankings, https://www.topuniversities.com/qs-world-university-rankings, and https://cwur.org/.

descending order. Universities ranked closer to the top of one of these ranked lists tend to appear on the other two lists. I relied on these global rankings for two reasons: to ensure I had a reasonably comprehensive list of universities, and to capture educational quality differences among job applicants who attended universities outside of the United States.

121. Universities at the top of the three rating systems tended to have unique rankings, in other words, there was one university ranked 1, 2, 5, 15, 25, etc. This was not always the case. Sometimes, two universities would be tied for a specific rank. I did not adjust these rankings. However, universities ranked further down, say below the top 500, tended to fall within ranges. For example, several universities might be ranked as "501-510" in a list. If the ratings system assigned a university a ranking which was part of a range, I assigned each university in range the midpoint of the range. For the example I provided above, if 10 universities were ranked "501-510" in one of the ratings systems, I assigned those 10 universities a rank of "505."

122. The names of universities on these ranking lists were similar, but not necessarily uniform. Each list had its own specific naming conventions: for example, whether to use the full name of a university or an abbreviation, or whether to use the vernacular or English name of the university. I used the Stata *matchit* algorithm to match the names of universities by name between the three ranking systems.[88] Once I matched the names of universities across the three lists, I had a full list of globally ranked universities based on the three ratings systems I mentioned above. Some universities only showed in one rating system, while other universities were featured on two or on all three ranking systems.

[88] Utilizing the *matchit* algorithm was straightforward in this case, because each ranking system gave both the name of the university and the country in which the university was located. I matched universities by name, but limited potential name matches on the ranking systems to universities in the same country.

123. I merged the rankings of universities with the list of universities and colleges that appeared on the job applications. I utilized the university ratings as separate controls in the starting pay regressions. I include all three rankings, with dummy variables indicating if a university was missing on a specific ranking. (See, e.g., Table B5, column 1.)

Appendix D: Using Prior Job Titles

124. When I study starting pay or starting level, I attempt to control for prior job experience. I based prior job experience on an applicant's prior job titles before working at Nike. Like for education, one approach is to create separate variables for the most common job titles in the data. (Here I do not create dummy variables, but measure experience in each job.) However, this inevitably leaves many job titles grouped together in a "catch-all" category. Moreover, there is a far greater number of job titles than universities – nearly 15,000 in the data. This is reflected in the fact that if I use all job titles with 10 or more observations, that accounts for only 30% of the observations (vs. 54% for universities). As a consequence, my preferred approach is to use machine learning (natural language) tools to cluster similar job titles together, and then to use those clusters of job titles as categories of prior job experience. The application of these methods to measure the "semantic similarity" between words or phrases is increasingly being applied in economics,[89] and these methods have begun to be applied in labor economics to classify jobs[90] and to study discrimination.[91]

125. Aside from avoiding the issue of a large "catch-all" category of unrelated job titles, my approach avoids the problem that if one tries to define a very large set of variables for different prior job titles, the estimated gender gap can become unreliable; the large number of different prior job titles may not capture any variation in the relevance of prior

[89] See the discussion of methods and examples in: Burn, Ian, Patrick Button, Luis Felipe Munguia Corella, and David Neumark. "Does Ageist Language in Job Ads Predict Age Discrimination in Hiring?" Forthcoming in *Journal of Labor Economics*.

[90] See: Jaeger, David. A, John M. Nunley, R. Alan Seals, and Eric Wilbrandt. 2020. "The Demand for Interns." NBER Working Paper No. 26729.

[91] See: Tian, Ye and Jingbei Zhang. 2021. "Employment Discrimination Analysis of Library and Information Science Based on Entity Recognition. *Journal of Academic Librarianship* 47(2): 102325.

experience, but because they may be correlated with being female, can absorb variation in pay that is properly attributable to gender. My approach limits the number of prior job titles used, while clustering these job titles into related jobs. And it puts all prior job titles in a cluster based on similarity, rather than leaving a huge category of prior job titles that get lumped together despite having no similarity. For both of these reasons, I view my approach as strongly preferred.

126. Prior to applying this method, much as I did for education data, I first had to standardize the names of prior job titles that I saw on an employee's job application. The data fields that contain information on prior career experience on Nike job applications are open-ended text fields, including the job titles. Thus, the same job may be represented in several different ways, contain spelling errors, or use abbreviations of common words. I had to account for these variations to arrive at a more concise, accurate representation of jobs.

127. This process required extensive data cleaning, and occasionally required correcting typos by hand. I made these changes and standardizations following specific rules as much as possible. Below, I discuss some examples. By following these rules when possible, and minimizing decisions about individual entries, I reduced the number of errors in job titles, and made the subsequent clustering analysis easier, in a fairly mechanical and neutral fashion.

128. I first went about systemically removing special characters from the job titles, and converting all the job titles to upper case. This involved several simple, straightforward processes, such as deleting double and triple spaces from job titles, removing special symbols (e.g., parentheses, slashes, dashes, colons), removing numerals, and removing and replacing accented letters (e.g., replacing "ñ" with "n," or replacing "ò" with "o").

129. With the job titles all capitalized and in a standardized format, I then set about removing obvious words that did not conform to a job title. For example, I removed the names of months that appeared in these job titles (e.g., "March," "October," etc.), or portions of web addresses (e.g., ".co.uk" or ".com").

130. Next, I replaced abbreviations in job titles. Some job applicants used abbreviations or shorthand to reference parts of titles or full titles. For example: "V.P." is shorthand for "Vice President," and "Dir" is an abbreviation for "Director." Whenever there was a common shorthand or abbreviation given on a job title on an employee's application, I use the full name instead.

131. I then corrected for common misspellings in job titles. As the name of a job title was from an open-ended text field, such errors were unsurprising. There are hundreds of examples I could draw from, but overall, the bulk of these misspellings and errors were from missing, duplicate, or transposed letters in a job title.

132. Finally, I corrected some typos by hand, after attempting to run the names of the job titles through a name matching algorithm matching job titles to other job titles. As I explain in further detail below, the name matching and clustering algorithm relies on a dictionary of words that form a complete corpus (i.e., "body") of text. If a given word does not show up in the corpus, it produces an error. Many of these words include the names of corporations, or conjoined words ("supplydirector" instead of "supply director"), or misspellings I did not previously capture. I removed as many of these errors as I could reasonably detect.

133. After this data cleaning, I removed any additional words from the job titles that are not found in the corpus used for the machine learning. Most of these remaining words that were removed from job titles appeared to be uncommon abbreviations.

134. The next step was to use these standardized job titles to compute semantic similarity between job titles, in order to put job titles in clusters that I would then treat as similar jobs. I used a corpus constructed from part of the Google News dataset obtained via the Gensim API. This corpus is fed into a neural network model that learns associations between various words in the corpus. Specifically, I use a *Word2Vec* model trained on this corpus to convert each job title into a vector of numbers that represent each word in the job title. From there, I can compute the similarity between two job titles using a metric called the cosine similarity score.

135. This leaves me with a matrix of numbers that reflect how similar each job title is to each other. I can use this information to segment linguistically-similar job titles into distinct groups or clusters. Unfortunately, this matrix is too big (14,735 rows by 14,735 columns, for the 14,735 unique job titles) to simply feed into a clustering algorithm. Instead, I use Principal Components Analysis (as implemented in the *sklearn* Python package) to take the information contained in the matrix and transform it so that I can capture most of that information using fewer variables, which are "components."[92] Following the common rule of thumb used in this technique, I choose the number of components such that the additional variation explained by each additional component begins to taper off substantially.[93] As shown in the graph below, this occurs at around 20 components, for which nearly 95% of the variance is explained. Thus, I use 20 principal components.

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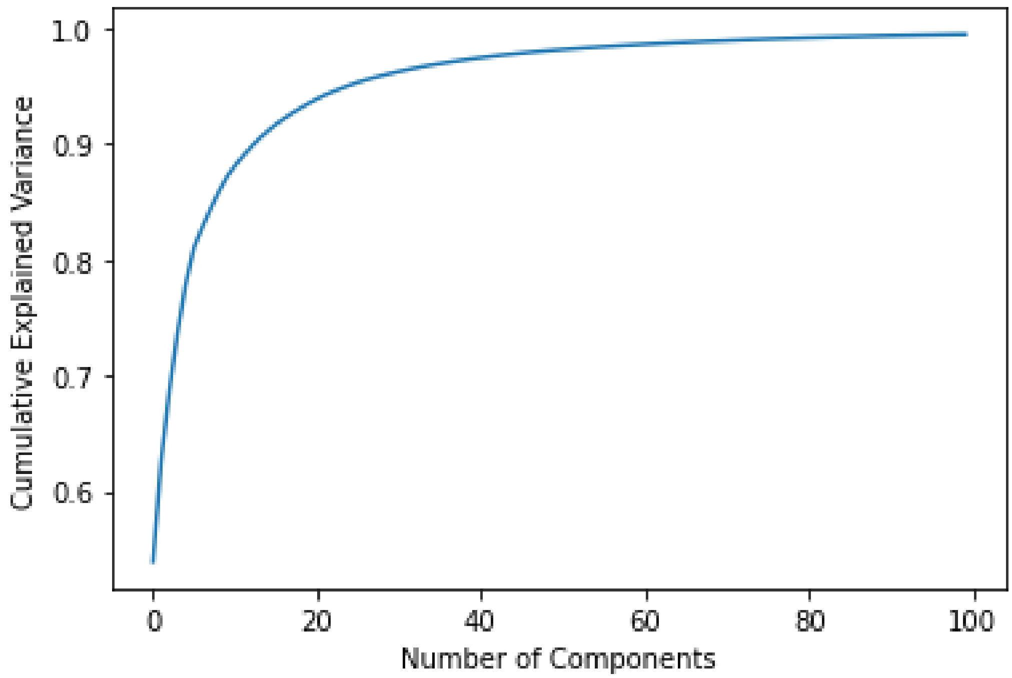
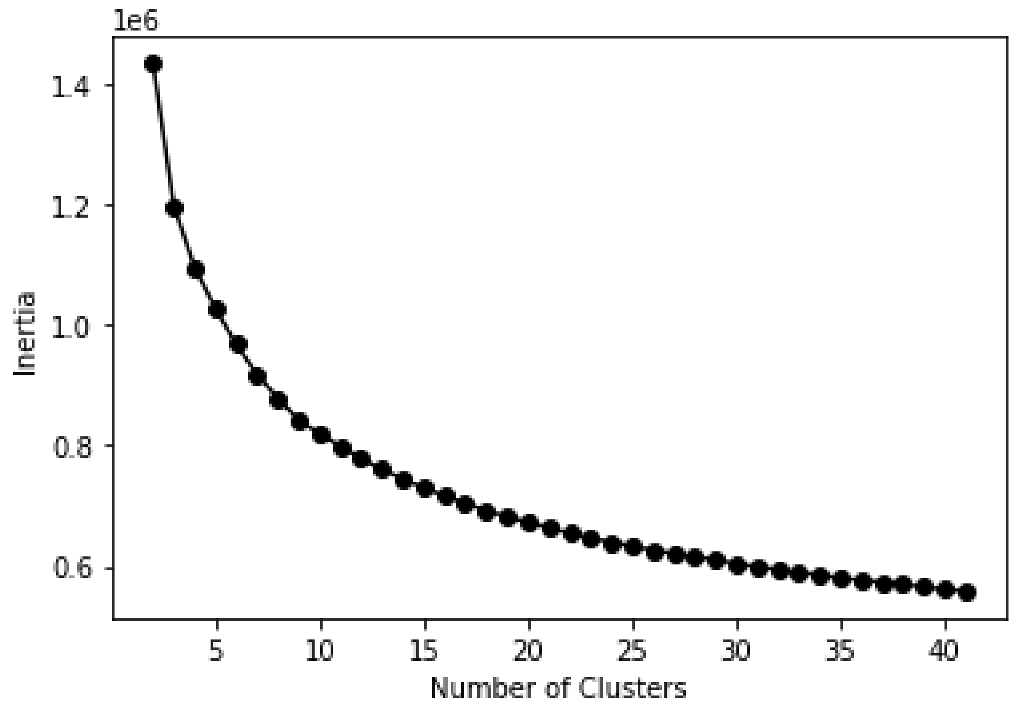
[92] See, for example: Thippa Reddy, G., M. Praveen Kumar Reddy, Kuruva Lakshmanna, Rajesh Kaluri, Dharmendra Singh Rajput, Gautam Srivastava, and Thar Baker. 2020. "Analysis of Dimensionality Reduction Techniques on Big Data." *IEEE Access*, Vol. 8, pp. 54776-88.

[93] See: Jolliffe, Ian T. 2002. Principal Components Analysis, Second Edition. New York: Springer. (Chapter 6).



136. I feed these 20 principal components into the *K-Means Clustering* algorithm implemented in the *sklearn Python* package, which then assigns the 14,735 job titles into clusters. To choose the number of clusters, I use another common rule of thumb. Specifically, I choose the number of clusters so that the reduction in the "inertia" (the within-cluster sum-of-squares) with each additional cluster tapers off.[94] As can be seen in the graph below, this occurs at around 20 clusters, so I assign job titles into 20 distinct groups.

[94] See: Thorndike, Robert L. 1953. "Who Belongs in the Family?" *Psychometrika*, Vol. 18, No. 4, pp. 267-76.



137. The evidence on starting pay (in Table 7) shows that controlling for these different job titles using this method *increases* the female shortfall in pay. This implies that when we add a lot of detail to capture the richness of prior experience, rather than *explaining* why women earn less when they start at Nike, the unexplained gender gap gets *larger*.

Appendix E: Publications from last 10 years

PEER-REVIEWED PUBLICATIONS:

Burn, Ian, Patrick Button, Luis Felipe Munguia Corella, and David Neumark. "Does Ageist Language in Job Ads Predict Age Discrimination in Hiring?," forthcoming in Journal of Labor Economics.

Neumark, David, and Timothy Young, "Heterogeneous Effects of State Enterprise Zone Programs in the Shorter Run and Longer Run," forthcoming in Economic Development Quarterly.

Neumark, David, and Luis Felipe Munguia Corella, "Do Minimum Wages Reduce Employment in Developing Countries? A Survey and Exploration of Conflicting Evidence," forthcoming in World Development.

Asquith, Brian, Judith K. Hellerstein, Mark J. Kutzbach, and David Neumark, "Social Capital and Labor Market Networks," forthcoming in Journal of Regional Science.

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Appendix F: Expert witness work in last four years

***Rabin et al. v. PricewaterhouseCoopers, LLP*, No. 3:16-cv-02276-JST, U.S. District Court, Northern District of California**

Serving as plaintiffs' expert witness to address statistical evidence on age discrimination in hiring. Deposed.

***EEOC v. Darden Restaurants, Inc.*, No. 15-20561, U.S. District Court, Southern District of Florida**

Served as plaintiffs' expert witness to address statistical evidence on age discrimination in hiring. Deposed and testified. Qualified as expert witness.

***Koehler et al. v. Infosys Technologies Limited, Inc., and Infosys Public Services, Inc.*, No. 2:13-cv.885, U.S. District Court, Eastern District of Wisconsin**

Serving as plaintiffs' expert witness to address statistical evidence on ethnic discrimination in hiring, promotions, and terminations. Deposed.

***Heldt et al. v. Tata Consultancy Services, Ltd.*, No. 4:15-cv-01696, U.S. District Court, Northern District of California**

Served as plaintiffs' expert witness to address statistical evidence on ethnic discrimination in hiring and terminations. Deposed and testified. Qualified as expert witness.

***Smiley v. Hologic, Inc.*, No. 3:2016cv00158, U.S. District Court, Southern District of California**

Served as plaintiffs' expert witness to address reasons for inability of plaintiff to find new employment after termination. Deposed.

***Jewett et al. v. Oracle America, Inc.*, 17-CIV-02669, Superior Court of the State of California**

Served as plaintiffs' expert witness to address statistical evidence on sex discrimination in pay. Deposed. Qualified as expert witness.

EEOC v. R&L Carriers, Inc. and R&L Carriers Shared Services, LLC, No. 1:17-cv-00515-SJD, U.S. District Court, Southern District of Ohio

Served as plaintiff's expert witness to address statistical evidence on sex discrimination in hiring. Deposed.

***Ellis et al. v. Google LLC*, No. CGC-17-561299, Superior Court of the State of California**

Served as plaintiffs' expert witness to address statistical evidence on sex discrimination in pay. Deposed.